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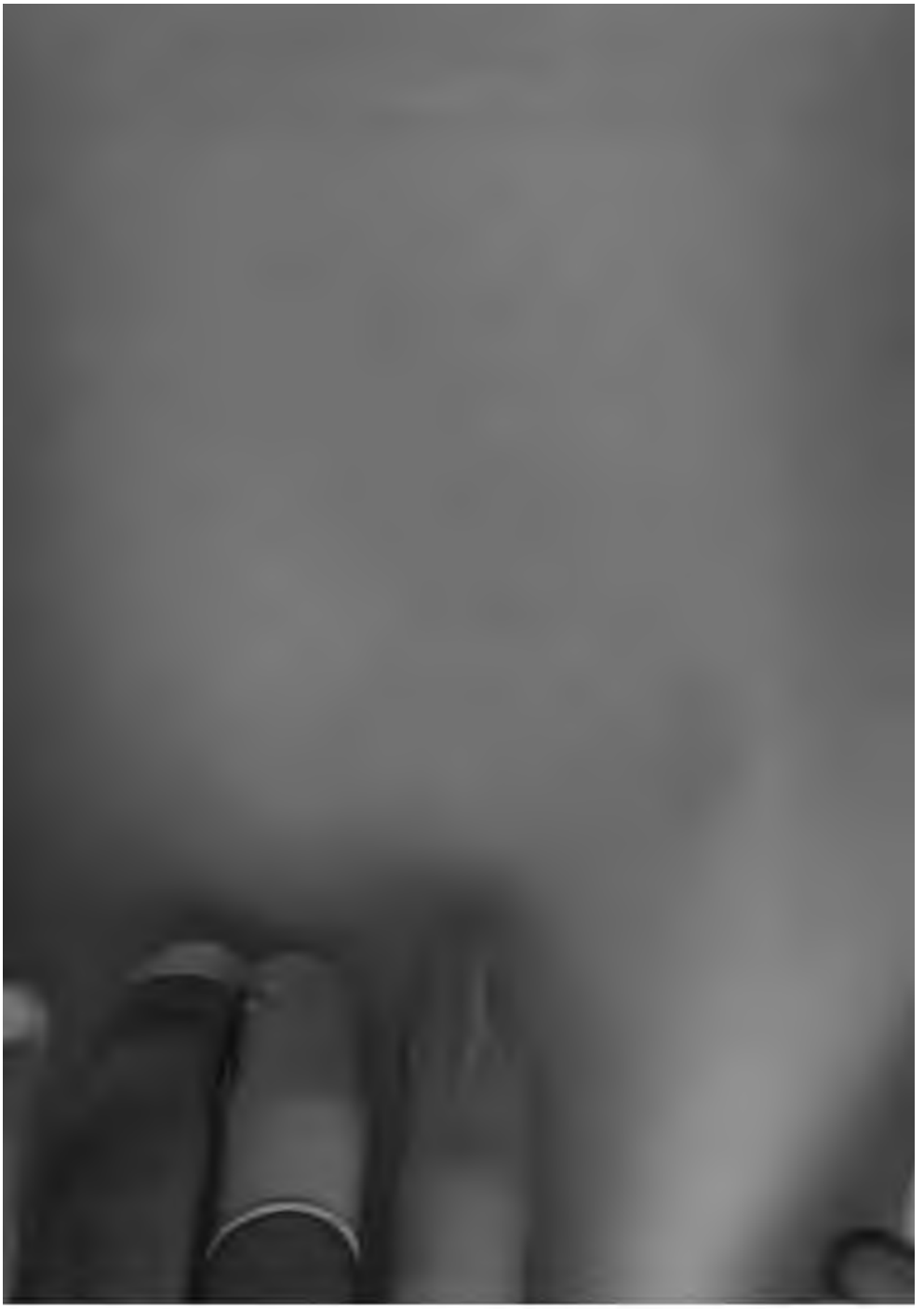

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OF

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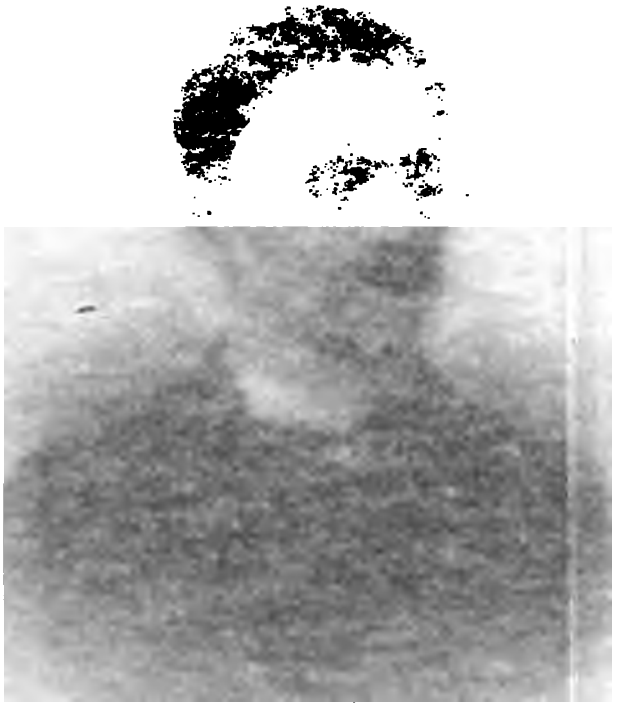

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OF THE

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

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SPECIAL MEETING, DEC. 16th, 1890.

The meeting was called to order at 8.15 P. M., by President W. A. Anthony.

In the absence of the Chairman Francis B. Crocker; Dr. Wm. E. Geyer, a member of the Committee submitted the report of the Committee on the Valuation of the Henry. [See Transactions, meeting of January 20th. 1891, page 31, for a copy of this report and discussion thereon.]

THE SECRETARY :—I move that the consideration of this report be deferred until the next meeting. I might give as one reason that the paper which is to be read to-night has some bearing upon it, and we have another Committee to report at that time, while our calendar is pretty full for to-night, and I think it would be well to defer consideration of it until January 20th.

[The motion was carried.]

Mr. Kennelly then read the following paper on "Inductance and its Proposed Unit the Henry."

A paper read at the 52d Meeting of the American Institute of Electrical Engineers, New York, December 16th, 1890. President W. A. Anthony in the Chair.

INDUCTANCE AND ITS PROPOSED UNIT THE "HENRY."

BY A. E. KENNELLY.

Perhaps the greatest discovery of Faraday's long career of scientific research was that of electro-magnetic induction in the year 1831, when he first brought to light the influence exerted by a magnet or a current upon surrounding matter when subjected to a change of magnetic potential. Oersted had shown that a conductor carrying a current exercised electro-magnetic influence at a distance, and Ampère had enunciated those simple and beautiful laws this influence is admitted to obey, but Faraday's discovery was a great step; it revealed the action of magnetic force in apparently empty space; it inaugurated that deeper insight into the mechanism, still only dimly guessed, which connects matter with all other matter in space; and it invested the all pervading ether with a dignity and power that in the hands of many great men since his time, has probably paved the way for our future comprehension of magnetism, gravitation, and radiant energy, while making the hypothesis of action-at-a-distance a mere mathematical conception and process of thought.

The development of this discovery of electro-magnetic induction has practically created electrical engineering, for it is to machinery operating by electro-magnetic induction that we owe nearly every branch of electrical industry except, perhaps, telegraphy, the forerunner of all; and the condition of modern electrical engineering is such that the units and nomenclature relating to steady currents and permanent magnets is insufficient for practical requirements, and the conditions developed by variable currents and magnets demand the establishment of engineering practical units for the purposes of convenience in expression and computation.

Foremost among these required units is that of inductance, or as it is commonly called the co-efficient of induction. This inductance is a property fully as essential to every electric conducting circuit as resistance and electro-static capacity, and the inductance of a circuit when conveying variable currents is often a far more important factor in regulating the flow than the ohmic resistance. In fact we know that Ohm's law does not apply to a circuit so long as the current is unsteady, unless the inductance of the circuit or the counter electro-motive force it involves be introduced into the expression.

In conformity with the conventional and so called absolute units of electro-magnetism, an inductance is a length and theoretically can always be computed from the geometrical relations of a circuit; that is to say, if the distance of each point from all other points of a circuit be measured, or inferred, in inches or centimetres, the inductance of the circuit will be some definite number of inches or centimetres, capable of being determined by a possible if not a convenient process of summation. If the circuit, however, be associated with iron or other magnetizable matter, then the magnetic permeability of the environment will enter the terms of summation as a multiplier. In the absolute centimetre-gramme-second system of units, inductance will be necessarily expressed in centimetres, and the centimetre is thus the natural scientific unit, although unsuited to the requirements of electrical engineering.

It may be well to notice that inductance is only conventionally and perhaps not essentially a length. Professor Rücker has pointed out[1] that the multiplication of the permeability factor with the length factor is always tacitly, if not expressly involved, even when the circuit considered is far from magnetizable matter in the ordinary sense; that is to say when residing in a medium of unit permeability, and therefore the nature of inductance is not merely length, but length multiplied by permeability, while physics has not yet decided whether permeability is the simple numerical factor at present assumed, or a function of length, mass and time. Until this question is settled, the real nature of inductance must be considered as doubtful, and the fact that inductance will probably, in any event, be always measured by lengths is no more validly an argument as to its nature, than the usual determination of a body's mass by the

1. *Phil. Mag*: February 1889. Fifth series, vol. xxvii, p. 104.

earth's gravitational force upon it is an argument that mass is force. However, the question is one of theory only, and do not affect the consideration of the practical unit.

As has been pointed out by Dr. Sumpner,[1] there are no le than three ways of defining the inductance of a circuit, or a po tion of a circuit, and the three definitions are equivalent whe the permeability of the magnetic medium is unity, but are n generally equivalent in the opposite event, being then only r lated in a definite manner by the magnetic properties of tl medium. In other words, the inductance of a hollow solenoid copper wire, placed in a vacuum and far from all iron, has tl same numerical value in each of the three definitions, but if a iron core be inserted in it, its inductance will depend upon tl definition selected, and will only be capable of interpretation fro

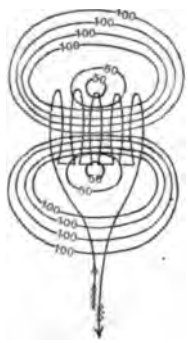


Fig.1

its value in one to its values in the others, by taking into accou the characteristic permeability curve of the iron, and the conditio of the measurement.

For convenience and general use, however, the secondary de nition of Sumpner and Fleming[2] is probably the best to adop and valuations in the other two can be reduced if necessary to th when the data for so doing are available.

The inductance of a circuit or portion of a circuit, is then tl ratio between the total induction through it to the current produ ing it. Thus taking a simple helix of five turns carrying a curre of two units, and assuming that 1,000 lines of force,[3] passed throug

1. *Phil. Mag*: June 1888. Fifth series, vol. xxv, p. 453.
2. "The Alternate Current Transformer" by J. A. Fleming, vol. i. p. 5
3. The term line of force here is used in the sense of a line of inductio the distinction being necessary when the lines pass through iron.

the central turn, of which owing to leakage, only 900 thread the next adjacent on each side, and again only 800 through the end turns, there would be 800 + 900 + 1,000 + 900 + 800 or 4,400 linkages of lines with the wire, and this being with 2 units of current there would be 2200 linkages with unit current, and consequently the self inductance of the helix would be 2200 centimentres. [Fig. 1.]

The fact that such helices are used in galvanometers, and that the magnetic force at any point within them is admitted to be proportional to the strength of the traversing current, renders it evident that had the current in this case been one unit, 2200 lines linked with the coil would actually have been produced. Supposing, however, that the helix had been wound upon an iron core, the total number of linkages with the same two units of current would have been greatly increased, perhaps twenty-fold or to 88,000 and the inductance then by the same rule would be 44,000. In this case one unit of current would not in all probability reduce the number of linkages to 44,000, the exact number would depend upon the characteristic curve of the iron core, and the degree of saturation, so that it would now be necessary to state the conditions of the measurement along with the result, in order to convey a clear and definite meaning.

It appears anomalous at first sight to state the above inductances in centimetres when they are derived from the number of lines of force, but it is to be remembered that the total number of linkages is divided in each case not by a mere numeric, but by a current, and this quotient can be proved to be a length in the existing system of dimensions.

In the same way the mutual inductance of two conductors is the ratio of the total induction through one of them to the strength of current in the other producing it. Suppose two coils of copper wire to be held in free space far from iron, and let one of them be traversed by a steady current of say four units. Of all the lines of force issuing from this coil, suppose 500 pass through the other coil. For simplicity, imagine the second or recipient coil to be composed of eight turns so disposed that the 500 lines pass through all in succession. There will then be 4,000 linkages of lines with that coil, and the mutual inductance of the two will be 4,000 ÷ 4 or 1000 centimetres. It is interesting to notice that the same result would be reached if the second coil had the current and the linkages were counted in the first, the

pair being mutually conjugate in this respect provided they reside in a medium of uniform permeability.

Thus far we have only considered the centimetre or absolute unit, whose unfitness for practical use is evident, since the smallest inductance that is at present known to engineering amounts to several thousand, while the average values met with are counted in millions of centimetres. It would of course be possible to name suitable units from the metrical system of lengths, but it is the proposal of this Institute to adopt the word "Henry" as the practical unit, in honor of the man, who beyond all doubt, was among the foremost to extend the knowledge thus far possessed of self and mutual inductance.

If the selection of the practical unit were entirely free and subservient only to practical requirements, so that its magnitude should express ordinary values in digits with fewest decimals on the one hand, or large numbers on the other, there seems most reason to believe that the thousandth of the earth-quadrant or myriametre would be the most eligible.

Proposed Name	Length.	Number of Centimetres.	Unit.
Henry	Earth Quadrant.	1,000,000,000	Practical Unit.
		100,000,000	
		10,000,000	
Millihenry.	Myriametre.	1,000,000	
	Kilometre.	100,000	
	Hectometre.	10,000	
Microhenry.	Decametre.	1,000	
	Metre.	100	
	Decimetre.	10	
	Centimetre.	1	Absolute Unit.

Of course large iron machines employing many turns of wire linked by dense magnetic fields, would have inductances more appropriately expressed in earth-quadrants, which might then be termed the "kilo-henry" just as we now have the watt, and kilowatt, but there is one restriction which makes such a selection undesirable, and that is the hitherto unbroken system of practical units itself. This practical system is a sub-scheme of the great international C. G. S. system and especially planned for adaptability to practical requirements. It is derived from that parent system by the substitution for the unit of length, one thousand million centimetres in place of one centimetre, and for the unit of mass an impalpable entity definite only in thought, the one-hundred millionth of a milligramme in place of the gramme. By this substitution in the C. G. S. electromagnetic units, we obtain the hitherto unbroken system of the volt, ohm, ampere, coulomb, joule, watt, and

farad. Even these units are not always the most convenient, witness for example the micro-farad in common use, whose prefix is maintained simply in deference to the harmony of the system, for the farad is a capacity that exists only in imagination; the capacity of the earth itself being only about 700 microfarads, and of the sun, or rather a sphere having the apparent dimensions of the sun being only about 76,000 microfarads. On the other hand a proposition has lately been made to infringe the system by making the volt, the ampere and the watt, ten times their present values, on the ground of practical convenience.[1] However, at the present time, it would surely be unadvisable to attempt the precedent of infringing a system that has hitherto been supported by time, custom, and international Congress.

In this practical system, the unit of inductance must be 1,000 million centimetres. That is an earth-quadrant or the distance from the pole to the equator, measured on the meridian of Paris. Strictly speaking, it is only the quadrant of the French standard earth, which is about one-hundredth of one per cent. shorter than the acknowledged actual earth-quadrant, according to the best determinations, but the fact of the latter being 10,001 kilometres instead of 10,000, is a matter of precision in definition only and in no way affects the practical unit.

As regards the history of the subject, it would appear that prior to 1887, measurements and deductions were confined to scientific works, and were expressed in absolute units—centimetres. In that year—however, a paper was read before the British Institution of Electrical Engineers by Messrs. Ayrton and Perry,[2] who invented and developed a practical measuring instrument for the measurement of inductance, who, aided by Dr. Sumpner, made important contributions to the literature of the subject, and who proposed a name provisionally for the practical unit. That name was the secohm, a contraction of second-ohm, implying the fact that the product of the two was the length equal to the practical unit. They drew attention to the fact that since the Paris Congress of 1884 had fixed the standard legal ohm as the resistance of a column of mercury at zero Centigrade, 106 centimetres long and one square millimetre in cross section, whereas the true ohm

1. *The Electrician* of London Sept. 19th, 1890, p. 558, Report of British Association Discussion.

2. *Journal of the Institution of Electrical Engineers*, London, April, 1887, vol. xvi., p. 291.

was in all probability 106.3 centimetres, or one-quarter of one per cent. greater, the inductances measured in the manner they suggested from seconds and legal ohms, would be in terms not of the standard earth quadrant of 10,000 kilometres, but of legal earth quadrants 9,978 kilometres in length. The difference practically is not very great, but to avoid error or confusion in definition, they advocated provisionally the term secohm, in place of quadrant.

The Paris Congress of 1889 adopted the "quadrant" as the practical unit of inductance defining it as 1,000 million centimetres.

The practical range of inductances is second only to that of resistances, and the latter we know from a few microhms up to thousands of megohms. The smallest inductances are those of Hertz resonators which descend to the order of metres. Excluding these, however, the smallest are perhaps met with in measuring instruments intended to have negligibly small inductance, such as doubly wound resistance coils, Cardew voltmeters, and the like. These descend to the order of decametres, and can generally be expressed as such without prefixing decimal points. It is evident that the term microhenry would suitably apply to their designation. At the other end of the scale we find large induction machines linking dense fields with many turns of wire, and reaching inductances of hundreds or even thousands of henrys. Intermediate between these limits, there exists a large class of circuits having many turns but little iron, or clad in iron with few turns of wire, and these seem to be best expressed by the term millihenry, equivalent to the myriametre.

With the object of setting forth the advantages to be derived from the use of this nomenclature, it may be permissible to adopt it provisionally in describing the measurements that follow. These measurements have been made on different types of apparatus at different times and by different methods, so that they represent mean values from several observations in many cases, but allowance must be made for the variations of standard and type.

In telegraphy, the inductance of an ordinary Western Union 140-ohm relay is about three henrys with the armature drawn well back, and nine henrys with the armature pressed against the poles, in the latter case, of course, the magnetic reluctance of the iron circuit is a minimum, and the same current in the coils causes

three times as many lines to circulate through the iron. At the position of the armature in ordinary adjustment the inductance is about five henrys.

The inductance of an ordinary standard 10ω relay is from 200 to 500 millihenrys, according to the position of the armature relatively to the poles.

The inductance of an ordinary sounder is similarly from 25 to 50 millihenrys. These inductances are the values obtained with only a few milliamperes through the coils, and since the path of the induction is largely through iron, the inductance varies with the strength of the current used. Practically, however, the variation is small within the range of current at which instruments usually operate.

Submarine Telegraphy.

The inductance of a mirror speaking galvanometer of the ordinary type having 2,250 ohms was found to be 3.6 henrys.

Telephony.

The inductances met with in the ordinary long-distance telephone apparatus are as follows:

The call bell 80 ohms and 1.4 henrys.

The magneto armature 550 ohms, and from 2.7 henrys with the plane of the coil in the line joining the poles, to 7.3 henrys when the iron core joins the poles, the plane of the coil being then at right angles to the magnetic flux.

The induction coil primary 0.28 ohm and 3.5 millihenrys.

Secondary 164 ohms, and 734 millihenrys.

The mutual inductance between the coils 60 millihenrys.

The Bell telephone receiver 75 ohms and from 75 to 100 millihenrys. The removal of the diaphragm from an ordinary telephone of this type reduces the inductance about 35 per cent. All these inductances are to currents of a few milliamperes only. As regards the inductance of aerial line wires, it is to be regretted that measurements are not yet forthcoming, and indeed the experimental determination is attended with some difficulty, owing to static capacity and imperfect insulation. In the absence of direct measurements, theory indicates that the inductance of overhead copper wires depends upon the elevation above the ground as well as upon the diameter. In the case of iron wires as used in telegraphy, the permeability of the iron enters as an additional factor. The following table of inductances for copper wires, is

based upon a formula first given by Clerk Maxwell.[1] It is to be hoped that experimental evidence may shortly be collected corroborating these results:

TABLE OF INDUCTANCES OF COPPER AERIAL LINE WIRE, PER KILOMETRE AND MILE, FOR DIFFERENT DIAMETERS, AND ELEVATIONS ABOVE THE GROUND.

Diameter of wire.		Elevation above ground 400 cms or 13.1 ft.		Elevation above ground 700 cms or 23. ft.		Elevation above ground 1000 cms or 32.8 ft.		Elevation above ground 1300 cms or 42.7 ft.	
		Millihenrys or Myriametres.		Millihenrys or Myriametres.		Millihenrys or Myriametres.		Millihenrys or Myriametres.	
Centre-metre.	Inches.	per Kilo-metre.	per Mile.	per Kilo-metre.	per Mile.	per Kilo-metre.	per Mile.	per Kilo-metre.	per Mile.
0.10	0.039	1.986	3.106	2.109	3.393	2.170	3.493	2.222	3.576
0.20	0.079	1.848	2.974	1.960	3.154	2.031	3.268	2.083	3.352
0.30	0.118	1.766	2.842	1.878	3.022	1.950	3.138	2.002	3.222
0.40	0.158	1.709	2.750	1.821	2.930	1.892	3.045	1.945	3.130
0.50	0.197	1.664	2.678	1.776	2.858	1.847	2.973	1.900	3.058
0.60	0.236	1.628	2.621	1.740	2.800	1.811	2.914	1.863	2.998
0.70	0.276	1.596	2.569	1.709	2.750	1.780	2.865	1.833	2.950
0.80	0.315	1.570	2.527	1.682	2.706	1.754	2.823	1.806	2.906
0.90	0.354	1.547	2.489	1.659	2.669	1.730	2.785	1.783	2.870
1.00	0.394	1.526	2.456	1.638	2.635	1.709	2.750	1.761	2.834

The inductance of iron wires, in the absence of measurements, is almost too doubtful to justify calculation, being perhaps 10 times more than the inductance of copper wires under similar conditions. The difficulty in their case lies in assigning the proper value of permeability.

Electric Light and Power.

Dynamos and dynamo-electric machinery are of course generally characterized by large inductances.

The inductance of the field magnets of a dynamo may have any value depending on the size and voltage of the machine from one henry up to 900 henrys.

The inductance of an armature may similarly vary from 20 millihenrys up to 50 henrys between brushes. One millihenry is a fair average value of the inductance of one section of an armature. Armature inductances depend not only on the strength of

1. Clerk Maxwell, "Electricity and Magnetism," 2nd Edition, vol. i. p. 293.

the measuring current, but also upon the strength of the field, and whatever influences the permeability of the core.

The inductance of an alternating transformer varies from a primary of 400 millihenrys, a secondary of one millihenry and mutually 20 millihenrys with a transformation ratio of 20, up to corresponding values perhaps one hundred times as great.

APPARATUS. THE INDUCTANCE OF RUHMKORFF COILS.

The smallest medical coil in ordinary use has five millihenrys in its primary, 100 millihenrys in its secondary, and 20 millihenrys mutually between them.

A large induction coil 19 inches long and eight inches in diameter has 0.145 ohms and 13 millihenrys in its primary, 30,600 ohms and 2000 henrys (by two concordant methods of measurement) in its secondary, and 163 henrys mutually between them.

The inductance of a mirror galvanometer may vary from a few millihenrys up to 10 or more henrys according to its resistance. Two henrys is a fair average for an astatic mirror galvanometer of 5000 ohms.

An ordinary electric bell of 2.5 ohms resistance was found to have an inductance of 12 millihenrys.

There is one point in connection with the self and mutual inductance of induction coils and tranformers, that deserves attention owing to the confusion that occasionally arises concerning it. It may be, perhaps, best represented by a numerical example. Let a closed Faraday ring of iron, 100 centimetres in mean circumference and 20 square centimetres in uniform cross-section, be closely wound with a primary coil of 2500 turns in one layer, followed by a secondary coil of 7500 turns also in one layer. If now two amperes be maintained steadily through the primary, the magnetic force in the iron will be $4\pi/10$ times the ampere-turns per centimetre of the iron circuit or $4\pi/10 \times 2 \times 2500 \div 100$ or 62.85. The permeability of wrought-iron at this force being, say 250, the induction developed per square centimetre will be 250×62.85 or 15,713 lines, and the total induction enclosed by the primary coil, neglecting the space occupied by the wire itself will be $20 \times 15{,}713$ or 314,250 lines which traverse every turn of both windings. The linkages with the primary will be $2500 \times 314{,}250$, or in round numbers 786,000,000 and referred to one absolute unit of current (ten amperes), five times this number, or an inductance of 3,930 million centimetres, that is 3.93 henrys. Similarly the linkages of these

lines with the secondary will be 2,358 millions, and for ten amperes of primary current 11,720 millions of centimetres mutual inductance or 11.79 henrys. Next suppose the primary current interrupted, and a current steadily established in the secondary sufficient to produce the same magnetic force in the iron, thus avoiding the complication of a changing permeability. As the secondary has three times as many turns, the current effecting this will be two-thirds of one ampere. The total induction through the iron will now be the same as before and therefore the linkages will be equally numerous. The self inductance of the secondary will be $2{,}358{,}000{,}000 \div \frac{2}{30}$, or 35.370,000,000 centimetres, that is 35.37 henrys. The mutual inductance will similarly be $786{,}000{,}000 \div \frac{2}{30}$, or 11.79 henrys as before. It will be found on carrying the reckoning to the necessary degree of accuracy, that this mutual induction 11.79 is the square root of the product of the two self-inductances, 393×35.37.

Examination of the steps in this calculation will show that this proposition must hold for all closed magnetic circuits uniformly magnetized to the same degree when the space occupied by the windings is not taken into account. Under the opposite conditions of open and especially of short iron cores, with large winding spaces, the condition usually fails, and the mutual inductance may be greater or less than the square root of the self-inductances multiplied together.

Another reason in favor of selecting 1,000 million centimetres as the value of the practical unit or Henry, is the desirability of having the time-constant of an electromagnetic circuit expressed directly in seconds by its practical units as in the case of electrostatic circuits, rather than revert to a unit of inductance that would yield the time constant in milliseconds or other fractional period of time. The time constant of a simple electromagnetic circuit, a circuit having inductance and resistance but negligibly small electrostatic capacity, is defined as the ratio of its inductance to its resistance, that is of its henrys to its ohms. It is an important function, and its consideration presents some remarkable analogies between inductance and electrostatic capacity that it is interesting to trace.

Imagine an inductance coil C without iron to be supplied with a steady current from an electromotive force E of 50 volts. Let the inductance of the coil be three henrys, and its resistance 10 ohms. [Fig. 2.] Ohm's law determines the strength of the

current to be five amperes. Now short circuit the terminals of the coil very suddenly by a wire across its terminals of negligible resistance. The current in the coil c does not stop instantly, but under the name of extra current is maintained in the coil by reason of its inductance. According to theory, the last dregs of the current will never entirely vanish from the coil, although after the first few seconds the remainder is exceedingly small. The very first instant after short circuiting, the current continues at five amperes by its electro-magnetic momentum but being unsupported, its energy is lost thermally in overcoming the coil's resistance, and it immediately falls. In falling, the lines of force linked with the circuit and sustained by the current, are drawn in, cutting the coil as they recede, thus creating a counter E. M. F. tending to prolong the current, and this process of dwindling current with reduction of opposition from the reserve of inductance, descend together. The total quantity of electricity that will flow through the circuit under the conditions specified, may fairly be called the charge or discharge of the coil, and it can be readily proved that this charge is equal to the quantity that would be conveyed by the initial discharging current, acting uniformly during a time equal to the time-constant of the circuit. In this case the discharge commenced at five amperes, and the time-constant is 0.3 second, so that the total discharge will be that due to the flow of this current in 0.3 seconds, that is 1.5 coulombs.

To represent the case graphically, suppose the curve of discharge in time to be plotted as in the figure. Let the axis OX be be divided off into seconds, and the axis OY into amperes. [Fig. 3.] The discharge commences at five amperes, subsides rapidly at first, but at last so slowly that an indefinitely long time must elapse before it altogether disappears. The area enclosed between the curve and the axes represents ampere-seconds or coulombs to the amount of the total charge, and the proposition just stated asserts that if the current had continued at five amperes steadily for 0.3 seconds, the area of the dotted rectangle so generated YDTO would be equal to the total area enclosed between the curves and the axes, prolonged to the ultimate point of union.

Curiously enough, exactly the same law applies to the discharge of a condenser through a circuit of resistance but neglibibly small inductance. The time-constant of the circuit in this case, however, being its capacity in farads, multiplied by its resistance in ohms.

The curve of discharge is of the same type for inductances or capacities. It is logarithmic, and finds its prototype in the case of the discharge of water in time, from a tank having a faucet at the base, assuming that the rate of escape through the faucet at any moment is directly proportional to the pressure, or level of water then remaining in the tank.

The laws which control the outline of this discharge curve are identical for condensers or short circuited inductances, and are combined in the following enunciation:

The natural logarithm of the ratio of the $\left\{\begin{matrix}\text{full charge}\\ \text{initial current}\end{matrix}\right\}$ to the $\left\{\begin{matrix}\text{residual charge}\\ \text{discharging current}\end{matrix}\right\}$ at any instant is the quotient of the time during which discharge has been in progress by the time constant of the circuit.

In this statement two reservations are necessary.

The electrostatic circuit must be free from all appreciable in-

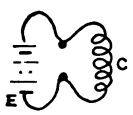


Fig. 2

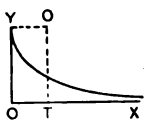


Fig. 3

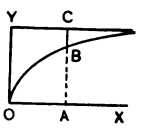


Fig. 4

ductance, and the electro-magnetic circuit must contain negligibly small electrostatic capacity. It was first pointed out by Sir William Thomson that the discharge of a condenser through an inductance circuit, becomes oscillatory when the inductance exceeds a certain critical value.

When an inductance discharges through a break instead of a closed circuit, the conditions become complicated to a degree that eludes complete practical solution in the present state of knowledge.

At the period of natural discharge equal to the time constant, an inductance or capacity charge or current will have fallen to 36.8 per cent. of its initial value.

Similar propositions relate to the charging currents and quantities. In the above instance, after the coil has completely discharged, remove the short circuiting wire. The current will immediately enter the coil, and the curve of its arrival in time is represented in Fig. 4, this curve being only an inversion of the preceding. For this reason it is evident that the area above the

curve now represents the charge, and by an extension of meaning, the deficit from full current, such as BC, may be called the charging current, since on discharge and reversal of the diagram, this will be the discharging current at the same lapse of time. In fact, the current AC is doing work in heating the coil, the deficit from ultimate current BC being only active in increasing the electro-magnetic momentum of the circuit, and work is really stored in the coil as though the deficit current did exist. A similar but inverted curve of current in time represents the charge of a condenser. In each case and under the above restrictions the following propositions hold:

The natural logarithm of the ratio of the $\left\{\begin{matrix}\text{full charge}\\ \text{initial current}\end{matrix}\right\}$ to the $\left\{\begin{matrix}\text{deficit from full charge}\\ \text{charging current}\end{matrix}\right\}$ at any instant is the quotient of the time during which the charge has been in progress by the time constant of the circuit.

When the charging interval reaches the time-constant, the charge will have attained 63.2 per cent. of its full value.

Another relation between simple electrostatic or electro-magnetic circuits and their time-constants is in their stores of energy. The energy of the charge in each case is half the energy that would be developed by the initial charging or discharging current against the ohmic resistance of the circuit in the time equal to the time-constant. In the above instance for example, five amperes being the initial charging or discharging current, and the resistance 10 ohms, the circuit energy of this current per second would be $5\times5\times10=250$ joules, and during half the time-constant the energy thus developed would amount to $0.3\times250\div2$ or 37.5 joules. This is the electro-magnetic energy stored up in the charge.

These are not the only analogies that exists between inductance and capacity, but there are remarkable points of diversity. The discharge of extra current of a coil is in the same direction as the circuit current, with the condenser it is the reverse. In a great many practical instances, the electro-magnet and the condenser are used to annul each others influence.

The time-constant of an inductance circuit is generally small for the reason that a coil of large inductance has generally many turns and therefore high resistance, so that the ultimate ratio of the henrys to the ohms is not large. For example, the time-con-

stant of a Bell telephone, considered in itself or on short circuit, is from the measurement above described, about $0.09 \div 75$ or 0.0012 seconds, and therefore a steady E. M. F. of say, two volts, impressed directly upon such a telephone would produce in this interval of time 63.2% of 2/75 amperes, namely 169 milliamperes. By adding inductanceless resistance to the circuit however, and raising the E. M. F. proportionately, the time-constant would be lessened, and the arrival of current accelerated. Strictly speaking this reasoning is inaccurate, since the telephone coil surrounds iron, but with the small currents usually employed, the iron probably does not greatly influence the arrival curve. In the case of dynamo field-magnets, and large iron-clad inductances generally, the divergence of the arrival curve from that of a corresponding simple electro-magnetic circuit is considerable.

There are several methods by which self and mutual inductance can be measured, the selection depending on the magnitude to be determined and its nature. The Ayrton and Perry secohmmeter or double revolving commutator is perhaps the most generally useful. It may be employed in connection with a standard condenser, a standard inductance, or apparent change of resistance with speed. Being used in the Wheatstone bridge, the currents it can alternate are necessary limited in strength.

For small inductances, [1]say less than one-tenth of a millihenry, the method of Professor Hughes[2] or its modification by Lord Rayleigh is probably best suited, using two coils of mutual inductance and a telephone as the observing instrument.

The inductance coils are usually in the form of two concentric rings, the inner being capable of rotation about the common vertical. The mutual inductance of such a pair of coils is a complex function of the dimensions and the angle between the planes at which the coils may be set. It is usual to calibrate the instrument by separate measurements of the inductance at different angles. A simpler arrangement however, for mutual inductance is a short solenoid in the bridge circuit superposed coaxially upon a long solenoid in the battery circuit. Adjustment to silence in the telephone for interrupted currents is then made by including in the bridge circuit a greater or less number of turns in the outer

1. *Journal of Institution of Electrical Engineers, London Journal, Jan.* 1886.

2. For another method of measuring small inductances with an alternating current, see an article by C. Steinmetz in the *Electrical Engineer*, (New York,) vol. x., Nos. 130, 131. Nov., 1890.

solenoid. The mutual inductance is then very nearly proportional to the number of turns so included, and by suitable proportions in construction, the accuracy can be carried as far as the telephone for the receiver will permit.

For the measurement of inductance in coils on iron machinery, the ballistic galvanometer is frequently employed, either by the method of discharge for self inductances, or induced quantity with mutual inductances. Mutual inductances connected by closed iron circuits present no difficulty when great accuracy is not required, since, as already observed, the self inductances are calculable in these cases when the mutual inductance and the number of turns in the coils are known. It is much more difficult, however, to measure the self-inductance of the field magnet of a dynamo when the armature is removed, and when currents of the normal exciting strength are used in the measurement. The best attempt has been made apparently by observing the discharge effects of gradual small changes of a strong current passing through the coil, in circuit with a specially arranged Wheatstone bridge.[1] If it were of sufficient practical importance, however, to measure such inductances with powerful currents, a recording instrument might be tried, having a very light and swiftly moving pointer, leaving a trace of its motion on a strip of moving paper like Sir William Thomson's siphon recorder. If such a pointer were arranged to give an amplitude of movement at right angles to the paper strip, in proportion to the current traversing the coil, it would register the curve of discharge when the coil was short-circuited, in the manner described above. On a sufficiently enlarged scale, corrected perhaps for residual inertia in the moving parts, this curve would give not only the total quantity of discharge, but its deviation from the logarithmic line of simple electro-magnetic discharge, would give the means of determining the characteristic of magnetization in the iron.

Of far lesser importance than the selection of the practical inductance unit, but still well worthy of separate consideration is the selection of appropriate symbols to represent the unit briefly. It might perhaps be advantageous to represent the henry by a capital H, and the millihenry by the plain letter h. This notation would enable such an inductance as 50 henrys to be written as 50 H, and 50 millihenry as 50 h, The microhenry occurs so seldom that it would probably not need a symbol.

1. *Phil. Mag*: Feb. 1889. Dr. Sumpner.

All measurements of precision should employ true ohms and not legal ohms in the computations. The best existing value of the true ohm is 1.0028 legal ohms.[1]

The correction is practically so small that it can best be carried out by altering the nominal standard temperature of the resistance coils adopted. These temperatures should be increased by 9.4°C for platinum-silver, 6.5°C for german-silver, and 13.5°C for platinoid. Thus a resistance box with platinum-silver coils standardized for legal ohms (of 106.0 cms) at 15°C would give true ohms of 106.3 cms at 24.4°C as the best approximation, and this would be the standard temperature of the box for use in precise measurements of inductance.

THE PRESIDENT:—I think there will be no difference of opinion as to the value of this paper and that the discussion of it will be of the very greatest interest. But it has been suggested, in view of the fact that we have another paper of great interest coming before the meeting, which is likely to be of some length, and also that we have just voted to postpone the consideration of the report of the Committee upon the Value of the Henry, that the discussion of this paper also be deferred until the next meeting, which takes place on the 20th of January, when it is hoped that a very full discussion will be had; and if there is no objection we will therefore consider that this discussion be postponed until that date.

I will now call for the next paper on the programme—"The Improved Gramophone," by Mr. Berliner.

1. Report of the Committee on Electrical Standards read before the British Association, Sept., 1890. See the London *Electrician* of Sept. 26th, 1890.

A paper read at the 52d meeting of the American Institute of Electrical Engineers, New York, December 16th, 1890.

THE IMPROVED GRAMOPHONE.[1]

BY EMILE BERLINER.

It might seem strange to you at first, that I should commence discussing the gramophone by talking about induction; but I would like to refer to a small portion of a paper which I read before the Franklin Institute in 1888 on the gramophone, when it was first shown, in which occurs the following:

"In my telephonic studies I had become familiar with all the causes influencing the transmission and reproduction of the voice, and what had at all times struck me as forcibly as anything in telephonic phenomena was the fact that the self-induction of long iron wires or of polarized electro-magnets acted so detrimentally upon the articulation. Electric resistance alone would simply have weakened the sound, but self-induction meant retardation and thus distortion of the transmitted waves which varied in length and amplitude."

And further on:

"In considering such, and other facts it became evident to me, that if such delicate energy as the voice, subdivided into, maybe, several hundred waves, should indent or engrave itself into a solid body, it needed but very slight mechanical resistance to modify considerably the character of the sound vibrations, for what self-induction is to the telephonic circuit, the variable resistance which impressible material offers to indentation or engraving at various depths is the phonograph record sheet. Neither is proportional in direct ratio to the expended energy, and must give cause, aside from a reduction in size of the sound characters, also to a distortion of the same."

And that brings me right to the subject of this evening, and I start by further explaining here, that those remarks had reference to the fact that in the phonograph it was impossible to make a loud reproduction which at the same time would be

1. The cuts illustrating this paper were kindly loaned by the *Scientific American.*

natural in tone. So long as you confine yourself to the small telephone voice the sound is natural and very satisfactory, but as soon as you attempt to indent at greater depth the waves are distorted, and the sound becomes unnatural. In the gramophone, as you will further on see, and I suppose you are perfectly familiar with the principle, there is no indenting and no engraving, but there is a constant resistance, that of the light pressure of an elastic blade on a flat surface, or on a cylinder, if you please, but whatever resistance there may be in it, is constant. And that is the principal difference, speaking scientifically, between the phonograph and the gramophone, that in one you have a variable resistance, in the other a constant resistance.

In May, 1888, before a meeting of the Franklin Institute, in Philadelphia, I showed publicly for the first time, how sound-

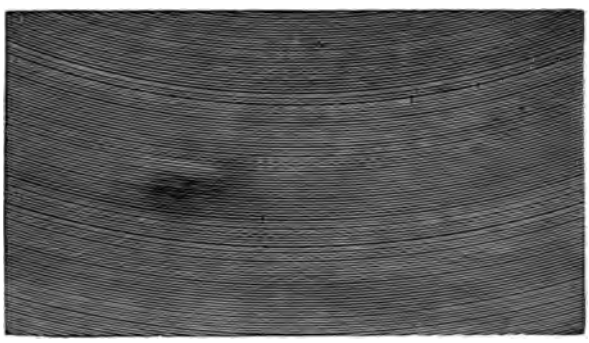


FIG. 1.—Gramophone Record.

records could be etched in metal, and how the original sound could again be made audible from such etchings. What I there demonstrated was of necessity crude in its effects, because embodied in the invention were a number of entirely new processes, new principles of construction and an art which dealt with motions infinitesimally small, the effects of which can not altogether be either seen or measured; all of which was brought to bear on the single problem of fixing forever and with as much resemblance to nature as possible, both in loudness and quality, the sounds of instruments, of noises of all kinds, and of that delicate and subtle form of energy, called the human voice.

At the same meeting, I read a lengthy paper dealing with the history of the twin arts, that of transmitting sound electrically, and that of recording and reproduciug the same mechanically.

That the development of both arts occurred as I then showed—in parallel steps, is not the only excuse for bringing talking machines before electrical societies.

In the gramophone, at least, there occur materials and processes, which partly or wholly electrical, comprise terms familiar to every electrical engineer. Zinc, acid, bichromate of potash, diaphragms, dampers, electrotyping, all these play their part in the technique of the gramophone, not to speak of the possibility of applying electric motors to drive the machines, and of using the electric current for rapid etching.

The science of electricity almost began with the immersing of zinc in acid, but crude force alone is thereby set free, while in the gramophone, the action of the acid on the metal is so curbed and regulated that under it the zinc becomes a picture of sound-waves which, though slumbering in a bed of hard metal, is ready at any time, even centuries hence, to burst forth into the soft cadenzas of word and song, the ripple of laughter, the strains of martial music, as well as the melancholy and imploring drag of the organ-grinder's tuneful melody.

The hydrogen which otherwise would be set free, is neutralized by the well-known depolarizer, chromic acid, and the carrier of the recorded sound-waves is so perfectly and yet so delicately insulated from the action of the acid, that a spiral of sound undulations 600 feet in length, could, without difficulty, be traced and etched on an 8-inch disc, and would represent when reproduced a continuous conversation lasting seven minutes; all this automatically, and with far greater facility than is possible in making a photograph picture.

In the art of etching, that which insulates the metal from the action of the acid is called an etching-ground, and in the gramophone, this etching-ground is a very delicate fatty film, deposited from the fluid state upon the metal surface. This fluid is made as follows: Into a bottle containing one quart of gasoline, benzine, or petroleum ether, are put 2 oz. of scraped, dark yellow beeswax, and allowed to digest, with occasional stirring, for several hours. We shall then find a clear yellow fluid on the top, and a white cheese like precipitate at the bottom. The clear fluid is decanted or syphoned off into another bottle, and diluted with the same quantity of gasoline.

When this fluid is poured on an absolutely clean and slightly warmed metal surface, there remains after the gasoline has

evaporated, a delicate fatty film, very porous, and so thin that it shines in iridescent colors. It is somewhat tenacious to the touch, but loses its resistance in a remarkable degree when alcohol is poured over it. As soon, however, as the alcohol is washed off with water, the film assumes again its tenacious quality with but very small loss in weight, due to its dissolving slightly in alcohol.

As this etching ground forms one of the keys to the whole gramophonic art, I will illustrate its qualities by the following experiment:

I have here a strip of zinc, over which the fatty film above described has been deposited from the solution of waxy fat. I have here also a piece of blotting paper, and with its edge touching the film, I now draw several cross-lines on the zinc. Next, I pour alcohol over it and draw a single line over the length of the strip,

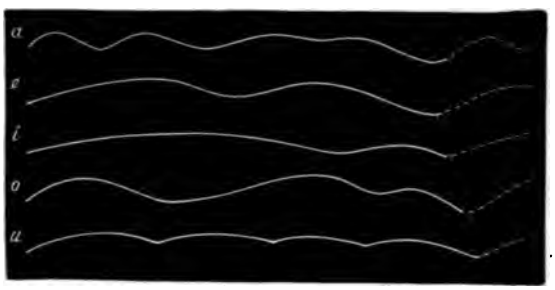


FIG. 2.—Gramophonic Vocal Records.

taking care to touch the surface as lightly as possible. I now wash off the alcohol and plunge the strip into this glass containing a solution of chromic acid, and you will presently see the long line appearing as a dark streak on the zinc, while the cross-lines appear less marked and more reluctantly, showing that the film offered less resistance under the alcohol than without it.

When after about 10 minutes, we take the strip out, we will find under a magnifying glass, that the track of each cotton fibre of which the blotting-paper consists, is sharply etched into the zinc; the rest of the metal has remained entirely untouched. such is the nature of the etching-ground used in the gramophone for etching sound records.

I will now illustrate how such records are produced, but I would like first to recapitulate briefly what I dealt with at length before the Franklin Institute, relating to the history of the art of making sound records of the human voice. That dates back

to the year 1857, when Leon Scott patented his phonautograph, which is described in every book on physical science. It consisted, as you see on that picture yonder, of a cylinder mounted on a screw, and turned by hand, a barrel-shaped mouthpiece, a diaphragm provided with a damping device, a flexible stylus attached to the centre of the diaphgram, and means for adjusting the position of stylus and record sheet to each other. The cylinder is covered with paper smoked with an oily flame, after which the stylus is so adjusted that it presses lightly on the smoked surface. When the cylinder is now revolved, the stylus traces a spiral line on the paper, and when at the same time,

Fig. 3.—Berliner's Improved Gramophone Reproducing a Record.

sound enters the barrel, the diaphragm will vibrate, and the spiral line assumes an undulatory form, according to the nature and pitch of the sound emitted.

Such tracings of sound are called "phonautograms," but since I have found that those published as having been made by the phonautograph are not true curves of the sounds they were supposed to represent, I have here some which are more correct, and drawn from gramophone records, representing some of the more prominent vowels. The waves are complex in character, and between 75 to 150 of these compound waves follow each other per second in the pitch of the ordinary human voice; all of which

is well known, and described in the chapters on sound of physical science.

In the spring of 1877, and following in the wake of the invention of the speaking telephone, it occurred to Charles Cros, of Paris, that if such sound-tracings could be engraved, for instance, by the photo-engraving method, and the stylus be then permitted to travel again in the groove or track of these engraved record lines, by revolving the cylinder past the stylus, the original sound would be reproduced. This was about six months before the in-

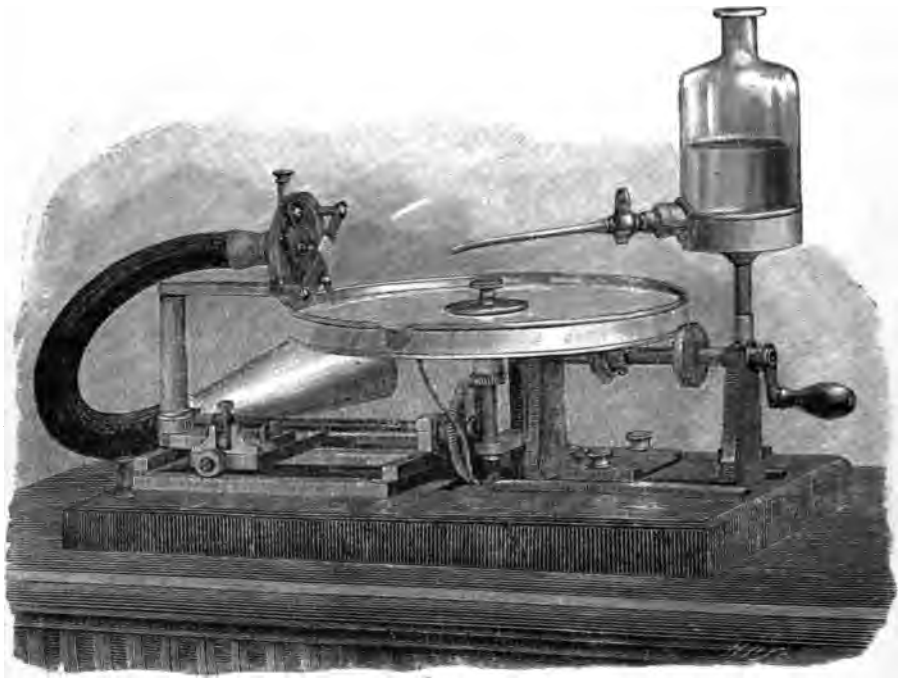


FIG. 4.—Berliner Gramophone Apparatus for Making Records.

vention of the phonograph, but the technical difficulties which appear in the attempt to bring about the desired result, must have seemed too great to Cros; or he must have become discouraged when, later on, seeing the apparently simpler method of the phonograph. At any rate, it appears that he never accomplished what he had conceived with such acute logic, and his paper on the subject (which can be found in the *Comptes Rendus*) was barely noticed, and quickly forgotten.

It was not until after the granting of the patent for the gramophone, that I happened to see a reference to it, and searching

the *Comptes Rendus*, found the article mentioned, which I then translated and embodied verbatim in my paper before the Franklin Institute. While, however, Cros had a correct conception of the general principle of a talking machine, it would have required considerable modification of the rules taught by physical science which were then at his command to obtain practical results; but I will illustrate what he meant, with this model built on the principle of the gramophone.

Here is a drum-head of oil-cloth, and across it and pivoted at the top, is an arm or stylus, which is connected to the centre of the drum, and therefore, moves with the latter. The free end of the arm extends beyond the periphery of the drum and carries a brush dipped in ink.

If I now pass a strip of paper underneath the brush and at the same time move the drum regularly to and fro, an undulatory line of regular character will be drawn on the paper. If I repeat the experiment, but more irregularly, or jerky, the motions of the drum appear marked down as a jerky irregular line.

I now replace the brush by a hard stick and permit it to travel in an undulatory groove engraved in a strip of wood, which strip I draw underneath the stick, and you will see by the reflection of light on the oil-cloth drum-head, that it is moved to and fro by the engraved wave-line, and if these motions should occur rapidly enough, a sound would be emitted by the drum.

This illustrates the general principle of the gramophone. It is, however, in the method of engraving the sound records that the gramophone departs materially from previous methods, and this I will now show you. Here is a disc of ordinary zinc, such as you can buy in every tinshop; it has been polished, carefully cleaned, and has then been coated with our fatty film. I now place the zinc on the turn-table of the gramophone recorder. The turn-table is revolved by a friction-wheel underneath, and connects through gearing with a screw, which propels a carriage across the turn-table, at a slow rate of progression. On this carriage is mounted the recorder proper, namely, a mica diaphragm, across which lies a spring, fastened above, and carrying at the lower free end a slender metallic blade, bent downwards slightly, and tipped with iridium, like a gold pen. This blade I now bring down upon the fatty film until it is straightened.

Mounted near the turn-table is a bottle containing alcohol, from which a spout extends to the centre of the turn-table, drop-

ping the fluid upon the fatty film on which I first pour some of it, so as to soak it.

If I now revolve the turn-table, the blade or pen marks a plain line into the fatty film, and as the iridium tip is kept free from the dust fibres which are continually floating about in the air, and upon the film also, by the presence of the alcohol, the line in proper light appears as a fine silvery streak in the dull fatty surface, showing that the metal is being laid bare by the iridium tip.

If, now, sound enters the tube leading to the diaphragm, the latter will vibrate and the line will assume the undulatory form of sound-waves, as before explained.

The gearing of the machine is at present set so that 72 lines are drawn to the inch; but for the ordinary conversational tone, the lines might be twice as close together, without the waves touching. The turn-table revolves at the rate of about 60 times a minute for this size of disc, and if they are smaller, the velocity must be increased, so as not to crowd the waves too closely upon one another.

The talking being done, I lift the disc from the machine and dip it in water, which washes off the alcohol, and there is now visible a band of silvery lines in the fatty film. I may now write the date in the central portion of the disc, and press the patent stamp on it, or add any other remark or signature, all of which will be subsequently etched in, and I may now plunge the disc into the bath of chromic acid. Immediately, the acid attacks the zinc in the lines, and a dark cloud forms over the record, due to the formation of a subchromate of zinc.

While the disc is being etched, let me now turn to the reproducing machine. The principal part is again a turn-table moved by a friction wheel underneath. Above it is an arm pivoted with one end upon a standard having a double or universal motion, and the other end carrying the reproducer proper, namely, a diaphragm of mica, vulcanite fibre, rubber or ivory, across which is fixed at its upper end, a stiff spring connected by a post to the centre of the diaphragm, its free end extending beyond the periphery, and carrying a small steel needle which may easily be taken out and replaced when necessary. The pivoted arm is the invention of Mr. Werner Suess, of Washington.

One of the mechanical curiosities of the gramophone is the fact that the etched record itself is the screw which propels the

diaphragm from periphery to centre, for the stylus resting in the groove by gravity or slight pressure, not only is vibrated, but following it and being able to move freely, is led along to the centre and to the end of the etched record automatically. This places the gramophone reproducer in the realm of extreme simplicity, and beyond the necessity of repair under ordinary every day conditions.

Before proceeding further, I will now let the gramophone show you some of the results of what it is capable of doing at the present time. I should like to remark, that in the present form of the resonator, the sharpness of the effects is somewhat toned down. That this is due to the resonator will become more apparent to you when, later in the evening, you will listen to some of the discs through the rubber ear-tubes. It has been proposed to use a resonator in the form of a large parabolic reflector, so as to enlarge the effects without modifying the timbre. But the effects, although loud, show the natural qualities of the various sounds, and are, in spite of their loudness, neither rasping nor ventriloquistic in quality.

The possibilities of extending the gramophonic principle, are perhaps more noteworthy than its present development. The discs can be easily duplicated, and at the first exhibition in Philadelphia, I showed an electrotype copy of a 12-inch disc which sounded precisely like the original. Since then, I have also succeeded in making talking copies by pressing a matrix into molten glass, but the matrix being of copper, the glass used to stick to the form, and warped the glass copy. I am assured, however, that whenever I shall furnish a steel matrix, the perfect copying in glass will be entirely feasible. That such steel matrices can be made is not doubted by those familiar with the art of transferring lines, and then etching the same.

We may then have dinner-sets, the dessert-plates of which have gramophone records pressed in them, and which will furnish the after-dinner entertainment when the repast is over. Gramophone plaques with the voices of eminent people will adorn our parlors and libraries.

Very successful copies have been made in celluloid from electrotype matrices, and such celluloid copies are particularly free from all frictional noise, provided the celluloid is pressed hard, and of well-seasoned material. Gramophone records have been

printed, and such prints have been photo-engraved, and the copy thus obtained sounded precisely like the original.

Successful copies have been made by letting one disc sing or talk to another one by coupling the recorder to the reproducer by a tube, and turning both simultaneously. In fact, this whole matter of duplicating discs is a department in itself, and would require a separate evening to do it full justice.

The work of gradually bringing the gramophone up to the present state has been exceedingly tedious. Working out telephones or transmitters is child's play in the face of the traps and Jack-o'-lanterns which beset the experiments with talking machines. The size, form and material of the mouth-piece, the density, length and diameter of the speaking tube, the size, thickness and material of the diaphragms, the tension, temper and thickness of the springs, shape of the needle points, temperature of the room in which the discs are coated, the quality of the beeswax, the strength of the acid, and the method of manipulating the sounds of voices and of various instruments—all these gave rise to errors and pitfalls, which only continuous repetitions of whole series of tests could locate, avoid, or obliterate.

I have to dwell on these facts, in order to ask your indulgence in not having been able for this evening to cover the important field of copying records—but I have also been disappointed in not receiving a number of celluloid discs, such as are now being made in Germany.

The important subject of good articulation has ever been kept in the foreground, and this is now in so satisfactory a shape that I am carrying on a vocal correspondence with my friends in Europe, by means of small gramophone discs, which can be mailed in a good-sized letter envelope.

Foremost among the feats which the gramophone can perform is the absolute certainty with which it enables people to recognize the speaker's voice, and I could cite a number of instances where persons have been made happy by hearing and recognizing the voices of loved ones whom they had not seen in years, and the owners of which were thousands of miles away.

This whole art is now manipulated with great certainty, and can be learned much easier than the art of photography. Yet, favorably as I believe the gramophone compares with other talking machines, it has barely entered upon the possibilities which lie dormant within its principles, only awaiting the touch of investigation to yield new and important scientific data.

Its advantages at the present time can be summed up as follows:

1. The records are durable, and require a minimum of space for storing.

2. Recognition is perfect.

3. The mechanism of the reproducing machine is a model of simplicity.

4. The records can be printed, and from such prints copies can be made which will sound like the original.

5. The quality of the sounds does not become impaired with increasing loudness.

6. The making of copies is possible by several well-known methods in existing arts.

Lastly, I ought to again refer to the possibilities of enlarging the record lines by the photo-engraving process, and thereby rendering the sounds with increased loudness.

Whatever the art of gramophony represents at the present time, does not consist of accidental results, but the principles and the errors to be avoided are well-known and established, and not a month passes but that new light is thrown on hitherto obscure sections of manipulation, and after three years of work, in which numberless sources of error have been eliminated, the art of etching records has lost none of its attraction, rather fascinating the mind by presenting the possibility of unlimited applications in the service, and for the pleasure of mankind.

Mr. Francis Forbes:—Perhaps a vote of thanks would first be in order, but I wish to offer this motion and I wish that somebody who is better able than myself to do so will offer the other. I move that this resolution be referred to a committee of three to report at the next meeting:

Whereas.—Under the patent laws as now existing, the United States patent of a citizen, is in danger of being limited if he try to obtain protection and compensation abroad, and

Whereas.—Said laws are unjust to the citizen of the United States and offer greater advantages to foreigners not domiciled in the United States.

Resolved.—That it is the sense of this Institute that the patent laws be so amended as to encourage the inventor who is a citizen to obtain patents abroad and further,

Resolved.—That the patent laws should be so amended as to render the rights of the citizen of this country in a foreign country and of the citizen of such foreign country in this country reciprocal.

[The motion was carried, that the resolutions be referred to a Committee of three.]

THE PRESIDENT:—The Chair would appoint as a Committee for considering this resolution, Francis Forbes, G. H. Stockbridge and H. C. Townsend.

THE SECRETARY:—I move a vote of thanks to Mr. Berliner of Washington who has so thoroughly explained and demonstrated the practical workings of his ingenious invention, and also to Messrs. Geo. H. Guy, and Charles McL. Paine who have volunteered their services, providing the musical features of the entertainment, and to Mr. Kennelly for his valuable paper upon a most important subject, which will add greatly to a thorough understanding of the question now pending before the Institute as to the proper value of the henry.

MR. T. D. LOCKWOOD:—I second that motion most heartily, and I believe we ought to present our vote of thanks to everyone who offers his services in that capacity here.

[The motion was carried.]

Adjourned.

The 53d meeting of the American Institute of Electrical Engineers, New York, January 20th, 1891, President W. A. Anthony in the Chair.

INDUCTANCE AND ITS PROPOSED UNIT THE "HENRY."

DISCUSSION OF MR. A. E. KENNELLY'S PAPER ON THE ABOVE SUBJECT, READ AT THE 52D MEETING, AND ALSO OF THE COMMITTEE'S REPORT ON THE PROPER VALUE OF THE "HENRY."

THE PRESIDENT:—[PROF. W. A. ANTHONY.] As we are to have the discussion this evening on the report of the Committee on the value of the proposed unit, I will call first for the reading of that report, and then we will take up the discussion of Mr. Kennelly's paper, and let the remarks embrace the report at the same time, as they will necessarily cover much the same ground.

The following report of the Committee was then read by Mr. Francis B. Crocker, Chairman:

REPORT OF THE COMMITTEE ON THE VALUE OF THE "HENRY."

At a meeting of the American Institute of Electrical Engineers, held September 16th, 1890, a committee consisting of Dr. Wm. E. Geyer, Dr. Louis Bell, George M. Phelps, Edward A. Colby, and Francis B. Crocker, Chairman, was appointed to consider and report upon the best value for the "Henry"—the proposed practical unit of self-induction or inductance.

After careful consideration, the Committee has reached a conclusion and now recommends to the Institute the retention of the present value of the practical unit which is 10^9 absolute units of inductance, usually expressed as a length equal to one earth's quadrant or 1,000,000,000 centimetres.

The Committee finds that this value is too high in some cases for convenience, as many practical measurements of inductance would have to be expressed in fractions of a henry. At the same time other practical values would often amount to a num-

ber of henrys, and sometimes to hundreds, or even thousands of henrys. It is of course impossible to have a unit fit all cases, and in this instance it happens that the average practical inductance would approximate one henry in value. This may be considered fortunate. Small values may be conveniently expressed in milli-henrys, analogous to milli-amperes, etc.

The Committee is of the opinion that even if the value of this unit (10^9 absolute units) had been much less convenient than it is, nevertheless it would probably be very unwise to change it, for the reason that it is derived from, and related to, the other practical units in the ratio of one to one, just as one ampere is equal to one volt divided by one ohm. To depart from this plan which has always been followed heretofore, would tend to destroy the harmony of the entire system of practical electrical units. The same fact made it necessary to define the farad as a capacity of one coulomb at one volt, although this makes its value so enormous that the micro-farad is always used in practical work. It appears, therefore, to the Committee that the value of the "henry" should certainly be fixed at 10^9 absolute units of inductance.

Respectfully submitted,

FRANCIS B. CROCKER, Chairman.

Mr. Crocker continued as follows: "I would like at the same time to mention one or two other points, the spelling of the plural of the name for instance. An awkward question has arisen, and I have taken expert opinion upon it. There is no doubt but that if it were a common noun, it would be spelled "henries" in the plural, but being derived from a proper noun, there is authority for spelling it "henrys," because the plural of proper names is sometimes given that way. Henrys, Marys, etc., are given by authority as proper plurals. I notice that it is spelled differently by different persons. Then the best definition of the henry in terms of other units comes up, but that is really to be settled by scientific usage. It can be defined in terms of second and ohm, or in terms of counter electromotive force and rate of change of current. But it is hardly necessary to settle upon any particular method, because each one will choose whichever seems best, but in regard to the plural, I think uniformity in spelling is necessary."

THE PRESIDENT:—The discussion of Mr. Kennelly's paper on inductance, and its proposed unit will now be in order, and it is understood that this discussion will also include that of the report of the Committee. The discussion will be opened by Dr. Cary T. Hutchinson.

Discussion.[1]

Dr. Cary T. Hutchinson:—Mr. President and gentlemen. The few remarks I have to make will be rather upon the subject of the Committee's report than of Mr. Kennelly's paper. I do not think there can be any doubt whatsoever as to the approximate value of this unit. It seems to me it ought certainly to be 10^9 (10 to the 9th) absolute units, but whether it should be exactly that, or only approximate to it, is another question—a point which Mr. Crocker has just touched upon in his notes, and which was not mentioned in the report itself. The whole thing hinges on what meaning the Committee intended the word "practical" to have. If by the practical unit is meant the unit corresponding to the ohm, and which is an even multiple of the C. G. S. unit, then it would have one value. If, however, by the "practical unit" you mean that value which will fit the ordinary equation of induction, of which Ohm's law is only a special case, the unit would have another value. In other words, the unit of current in the electro-magnetic system is defined absolutely; the legal ohm or whatever unit of resistance is chosen, is arbitrarily defined, and its relation to the ohm is determined experimentally. The unit of electromotive force is deduced from Ohm's law. Now the unit of induction might be defined as 10 to the 9th C. G. S, units or it might be defined as that value which will make the equation $r\,C + L\,\frac{d\,C}{d\,t} = E$, true: the two values will not coincide at all, for the simple reason that the ordinary unit of resistance is not the ohm. It is not what is called the "true ohm," although I think "true ohm" is a misnomer—the ohm is the ohm. It seems to me therefore, that this question should be gone into. The whole thing was discussed at considerable length a year or so ago in a series of letters which appeared in one of the English papers. The Paris Conference in 1889 recommended 10^9 centimetres, and named it "Quadrant." The British Association adopted both the value and the name. Professors Ayrton and Perry very vigorously maintained that the unit to be used by practical men must have the value which makes that equation of induction true, which would be approximately $.9977 \times 10^9$. The matter does not seem to have had discussion, at least so far as I know, and no mention whatever is made as to the definition of the unit. I think the definition in terms of the centimetre is unfortunate, for the reason that its dimensions being a length is purely artificial, and is so only on account of the "suppressed dimension." The definition should be in terms of the phenomenon; that is the counter-electromotive force, expressed in terms to suit the C. G. S. "practical" or "legal," as may be decided. No one can measure the actual inductance of the coil from its dimensions except in very special cases, and if measured in air

1. By Messrs. Hutchinson, Wolcott, Reid, Crocker, Kennelly, Mailloux Steinmetz, Wheeler, Anthony and R. W. Pope.

the result would be wrong with an iron core. All Mr. Kennelly's measurements were made experimentally; they were not calcuted from the dimensions of the coil. Nor do I altogether agree with the sense of the Committee on the question of the name. Of course this entire series of units has been named for distinguished men of science, but fortunately they all had names which were uncommon even in their own countries. Ampère was not Jacques, neither was Ohm, Heinrich, and the fact that "Henry" is such a common name seems to me a serious objection to it from this point of view. Undoubtedly Professor Henry ought to be honored, and honored very greatly, although I do not know that it is strictly true that he was the discoverer even of the bare fact of self-inductance. It seems to me that it has been shown recently that that is not quite true by Mr. Lockwood who brought forward a reference at the Boston meeting of this Institute, in which he said that in 1803 Vassali-Eandi discovered the increase of the spark on breaking circuit with increased length of wire[1].

Henry in 1832, discovered the same fact, developed it somewhat, and gave an explanation; but he dropped the subject there, and did nothing more with it at all until about 1838, some years after Faraday had taken up the subject and run it practically into the ground. Then Henry took it up again, developed it considerably, chiefly by showing the existence of currents of a higher order. In those subsequent papers he refers constantly to Faraday's work, showing that he acknowledged the indebtedness. He undoubtedly developed the actual discovery of the fact of the increase of spark on increasing the length of wire much further than Eandi.

However my most serious objection to the whole matter is lack of jurisdiction. It seems to me that the Institute is assuming a great deal to take upon itself the settling of such a matter. I do not think that this is in any sense a scientific organization, strictly speaking, and I believe that much the better way would have been to act in concert with the various other bodies which are just as much interested as we are in the subject - in fact a great deal more interested, ours being only a technical interest in a strictly scientific subject. A committee has recently been appointed in England, made up of representatives of a large number of institutions; the Royal Society, the Institution of Electrical Engineers, the British Association for the Advancement of Science, and two or three others; to consider the same subject. In this country there are bodies occupying somewhat the same relative rank as those do in England; for instance, the National Academy of Sciences, the American Philosophical Society, the Smithsonian Institution, and others. Before settling as far as we can, a matter that interests a large number of people besides ourselves, and which must meet with international consent to be of any value, would it not have been better, more

1. TRANSACTIONS, vol. vii. p. 229.

courteous, and a method which would have carried more weight, to invite delegates from these bodies to meet us and act with us? Such a committee on "Electrical Units and Nomenclature" could represent the United States with dignity, in an International Conference. Indeed it is not too late now to have a committee of this kind made up by invitation. It happens that at the present moment a committee has been appointed in England comprising Lord Rayleigh, Sir William Thomson, Carey-Foster, Glazebrook, John Hopkinson, W. H. Preece, and three or four others, which is to consider this very subject. Nothing better could be done than to appoint a body which would represent the United States, and not the American Institute of Electrical Engineers alone, and have the matter properly considered.

Mr. Townsend Wolcott:—Some of the points I have written here are so similar to those of Dr. Hutchinson, that I must ask to be excused for any apparent repetition. In his paper on Inductance and its Proposed Unit, the Henry, Mr. Kennelly remarks: "It may be well to notice that inductance is only conventionally and perhaps not essentially a length." He than quotes some remarkable statements of Prof. Rücker on this point; remarkable in showing the amount of misapprehension which exists among scientific men regarding so simple a subject as the theory of the dimensions of physical units.

Inductance is not a length; there is no room for a "perhaps" regarding that question To be sure the *unit* of inductance has the dimensions of the *unit* of length in the electro-magnetic system of units, but then in the electro-static system it has the dimensions $[L^{-1}\ T^{2}]$. Is there any more intimate association between the electro-magnetic system of units and the actual physical quantities which they represent, than between the electro-static system and the same physical quantities? If not, it behooves some learned professor to cudgel his brain over the question whether inductance is really the reciprocal of an acceleration, or whether it is more likely the reciprocal of a velocity "a quantity of the nature of a slowness," (as one of said learned gentlemen once remarked,) "multiplied by a time."

A slowness multiplied by a time must be very slow—too slow I fear for this audience, so I will say no more about it.

It is a fundamental and oft-repeated principle of logic which is still oftener disregarded, that because all A's are B's, it does not by any means follow that all B's are A's. Because all horses are animals, it does not follow that all animals are horses. Because all lengths have a unit denoted by (L) it does not by any means follow that all quantities whose unit has the dimensions of (L) are lengths. There is another logical principle which has been overlooked in this matter, namely: When any system of notation of measurement or other analogous artifice is adopted, there are adopted along with it a greater or less number of artificial properties or relations, as distinguished from the natural

properties or relations subsisting between the objects themselves which the given artifice represents or to which it applies. For example; numbers have natural, inherent, or intrinsic properties, thus: numbers are prime or composite, all square numbers are divisible by four, or else the next less number is divisible by four, and so on indefinitely. These properties are of course entirely independent of any system of notation, but there are other properties of numbers, which are of an extrinsic, artificial, (or if you prefer the expression) casual or contingent character; contingent on the particular system of notation employed. Thus if the digits of any number add up to nine or a multiple of nine, the number itself is divisible by nine. This is a contingent property in the decimal system of notation, of nine the highest digit, and three a factor of nine. In any other system of notation constructed on the plan of the decimal system, this property would still belong to the highest digit whatever it might be, and to its factors if it had any. In the decimal system any number ending in zero or five is divisible by five, and any number ending in zero or an even digit is divisible by two. These are the contingent properties of the factors of the periodic numbers, and would belong to the factors of the periodic number if such there were in any other system of notation

All the relations which may be established between the dimensions of units are of this contingent or artificial character, some depend upon the particular system of units under consideration, others may be the same in two systems, but still contingent on some particular supposition which is the same in both systems. A further illustration will show more forcibly how utterly groundless is the claim that because the unit of inductance has the dimensions of the unit of length, inductance therefore is length. So far as I have been able to ascertain, no one who knows the meaning of the terms has ever asserted that electric current and magnetic potential are identical. And yet there is more reason for this statement than the other regarding inductance, as the dimensions of electric current are identical with those of magnetic potential, not only in one system of units but in both the electrostatic and electro-magnetic systems. In the electro-static system the dimensions of C and Ω are $[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-2}]$ and in the electromagnetic system the dimensions of C and Ω $[L^{\frac{1}{2}} M^{\frac{1}{2}} T^{-1}]$. We know that every point in the neighborhood of an electric current *has* a magnetic potential, but that is all. We do not say that the electric current *is* the magnetic potential, or that the magnetic potential *is* the electric current. The reason why these two quantities have the same dimensions always, is simple. According to the theorem of Gauss, the magnetic potential at a point due to an electric current is numerically equal to the solid angle subtended at the point by the current, multiplied by the strength of the current. Now the solid angle is evidently independent of the units of time and mass, and

a very little consideration will show that it is also independent of the unit of length. The dimensions of magnetic potential therefore are the same as those of electric current regardless of the system of units, for in the electro-static system we first fix upon the dimensions of the unit quantity of electricity from which follow the dimensions of unit electric current and consequently of magnetic potential, from which we obtain the dimensions of the other magnetic units. In the electro-magnetic system we first fix upon the dimensions of the unit quantity of magnetism, (which happens to be the same as those of unit of electricity in the electro-static system owing to the fact that electro-static and magnetic forces follow the same law) and then derive the dimensions of the other units in the inverse order. I have gone thus into minutiæ because it seems to be necessary on account of the prevalence of misunderstanding on these points.

One of the aforementioned learned professors even gives us instructions for juggling with the units so as to form a mental picture of resistance as a velocity. If it really were a velocity, we could measure it with a meter scale and a clock, or at least with some of the known methods of measuring real velocities. Moreover it should be remembered that unit resistance in the electro-static system has the dimensions of a "slowness" (as it has been called.) It is sad to think that an unfortunate resistance is called upon to be a velocity and a slowness at the same time, just because two different men wish to measure it with two different units. If resistance were really a velocity in one system it would be in all. The physical nature of a quantity can not be changed by changing the system of units, but only the numerical factor which indicates the number of times the unit is taken to make the given quantity.

If inductance really were a length, not only ought we to be able to measure it by some of the methods of measuring length, but it would be absurd to encumber our nomenclature with a new name for an old unit. We should by all means stick to quadrant, or centimeter as the case might be. After what has been said, it seems almost unnecessary to remark that no discovery in science can change the dimensions of magnetic permeability, or of any other unit in either of the present systems of units. We might discover a better system of units, but that is all. For instance, if it were discovered that the magnetic effects of a current were not exactly proportional to the current in air or vacua we would not say that Gauss's and allied theorems are inaccurate, but that the permeability of air or vacua is not quite constant, and would pick out some standard conditions for which we would *assume* the permeability unit, and express the permeability under other conditions in terms of this. There would however be no change in the dimensions of the unit of permeability. As to permeability, or any other physical quantity being a function of length, mass and time, I would say that this phrase is another stumbling

block. It means, or ought to mean, that the unit of the quantity under consideration, may be chosen in such a manner as to depend upon the units of length, mass and time. That is, so that if the magnitude of the unit of length, mass or time be altered it will alter the magnitude of the unit of the quantity under consideration. This is all: it does not mean that length, mass and time are elements, as it were, after the manner of chemical elements which may be combined in various manners to produce all other physical quantities which are compound in their nature. And yet this is pretty near the idea which many persons entertain if we may judge from what they say. If Mr. Kennelly's or Prof. Rücker's mind is disturbed by the fact that magnetic permeability, although a physical property of matter, has no dimensions with respect to length, mass and time, I would call their attention to the fact that this is so only in the electro-magnetic system, while in the electro-static system the dimensions are those of the inverse square of a velocity, or if they so prefer, the square of a slowness. On the other hand the specific inductive capacity of a dieletric has no dimensions in the electro-static system, and the dimensions of the inverse square of a velocity in the electro-magnetic system. There is nothing more fundamental in length, mass and time, than in any other physical quantities whatever, and there is no other reason than convenience for assuming the units of these quantities as fundamental units. Those who will read Maxwell's chapter on units will there find more than a suggestion of the foregoing statements. Maxwell there shows that in the astronomical system, the dimensions of unit mass are $[L^3\ T^{-2}]$. Here we have only two fundamental units, length and time, but do we find any advocate of the theory that mass may be generated from length and time or that it is a "function of length and time?" I think not. Every one now-a-days seems to believe in the indestructibility of matter, which can not be true if mass itself can be resolved into length and time, and not merely the unit of mass made to depend upon the units of length and time. It is worthy of remark that in the first twelve of the eighteen electric and magnetic units of which Maxwell gives the dimensions, the exponent of M is uniformly $\frac{1}{2}$, and in the remaining six, (which are each the ratio of two of the other twelve) the exponent of M is of course 0. That is M disappears. If therefore we should adopt the astronomical unit of mass, the electro-static and electro-magnetic systems of units would still remain distinct. We are taught with our elementary arithmetic that two concrete numbers can not be multiplied together. At least one of the factors must be an abstract number, and in this case the product is of the same kind as the concrete factor. Consequently when we say that the area of a rectangle equals the product of its length and width, we mean that the *number* of linear units in the length multiplied by the *number* of linear units in the width equals the *number* of superficial units in the area. But even this

is a contingent property of the rectangle; contingent on the adoption as the superficial unit, of a square, the length of whose side is equal to the linear unit. It seems to me that this very well known principle is sometimes overlooked in speaking of electrical units. But further than this, when we say that a length divided by a time is a velocity, it is not necessarily true even when we mean it in this strictly numerical sense. A length divided by a time does not even numerically represent a real velocity, unless the numerator of the ratio represents a length which is actually traversed by something in the time which is represented by the denominator. The concept of a velocity involves the motion over distance in time, even if the motion is only that of a mathematical point. In order, however, that a velocity should have any physical significance, the motion must be either that of a material particle or some physically demonstrable action such as a light wave. If we divide the diameter of a millstone by the time it takes to grind a bushel of wheat the quotient would not in any sense be a velocity. Nevertheless, if it were true that the rate of grinding varied exactly inversely as the diameter of the stone, (which of course it would not do in practice) then the diameter of the stone divided by the time it took to grind a bushel of wheat would be a constant. This proposition may appear very absurd, but even Maxwell himself has employed a method of reasoning analogous to this. In article 628 he writes, "If the units of length, mass and time are the same in the two systems, the number of electro-static units of electricity contained in one electro-magnetic unit is numerically equal to a certain velocity, the absolute value of which does not depend upon the magnitude of the fundamental units employed. This velocity is an important physical quantity, which we shall denote by the symbol v." So far all is undoubtedly correct, and leads by inductive reasoning to the inference that v *may be* a real velocity. When, however, Maxwell undertakes to show in article 768 that v is really a velocity, he simply shows that the distance between two small electrified bodies, divided by the time it takes two given currents to charge them is a constant, if the distance be so regulated that the repulsion is a constant. Were it not for the place in which this is written, I should think that Maxwell intended to perpetrate one of those deep and ponderous, though quaintly humorous jokes which his versatile brain sometimes evolved. But being where it is, I have rather thought that I have misapprehended his meaning. However, the whole thing is so plain that I do not see how there can be any misapprehension. We have already seen that electric current has always the same dimensions as magnetic potential. There are two other quantities related in the same manner, namely: quantity of magnetism and electro-kinetic momentum of a circuit m and p have the dimensions in the two systems, $[L^{\frac{1}{2}} M^{\frac{1}{2}}]$ and $[L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-1}]$. We may therefore substitute m for p in any equation without altering either the dimensions or the

numerical value of the other quantities. It is on this principle that Sumpner and Fleming's definition of inductance is founded. Maxwell defines inductance as the ratio between the electro-kinetic momentum of a circuit and the current: that is, $\frac{p}{C}$. Sumpner and Fleming say the ratio between the total induction (the same as Maxwell's quantity of magnetism), and the current. This is simply substituting m for p in the ratio giving $\frac{m}{C}$ = inductance. On the same principle we may substitute for the current the magnetic potential at a point where the solid angle is numerically equal to the reciprocal of the permeability when the ratio becomes $\frac{m}{Q}$, the quantity of magnetism divided by the magnetic potential, the analogue of the quantity of electricity divided by the electric potential or electro-static capacity. Electro-static capacity has the dimensions of a length in the electro-static system and of the reciprocal of an acceleration in the electro-magnetic system, just the reverse of inductance. I will now leave the theory of dimensions with the single observation that judging from the amount of nonsense that has been written and spoken on the subject, 'tis but a step from dimensions to dementia. Now in regard to the size of the "henry." I am pleased to see the drift of opinion toward adopting the quadrant value for several reasons. In the first place "too many cooks spoil the broth": If every one who has an induction coil or relay, must have the henry made to fit it, so that its inductance may be expressed in one or two figures, we will have no unit at all. It has been complained for several years that there was a great diversity of wire gauges, but the controversy has added to the number in use to say nothing of the others recommended. It must be admitted that the Committee of the British Association did its work pretty thoroughly, and being a small body of men did what a large body finds more difficult, and the electrical community at large finds impossible. They agreed among themselves, and produced a system of units. Let us stick to the system. We are now struggling with three values of our present ohm and volt, and if we increase the values tenfold as has been suggested, to simplify matters, we will have six ohms, and six volts, by which time if we are not all candidates for the lunatic asylum we will think how much simpler it was in the good old days when everybody expressed E. M. F. in Daniell's cells. I consider the criticism that our volt is too small, as very weak. The highest E M. F. used so far, in practical engineering is I believe, 10,000 volts or thereabouts. I have never heard a bridge-builder complain that a foot is too small a unit because he builds bridges several thousand feet long. I have never heard any one complain that the dollar is too small a unit because he can't ex-

press his bank balance in less than five, or even seven figures. In fact we already have a larger legal unit, the eagle, but no one uses it; it is merely the name of the ten-dollar gold piece; not even of the ten-dollar bill. In machine shops, although we nominally have feet and inches, the practical unit is the inch. We never hear a machinist speak of a steam cylinder of eight feet stroke, but of ninety-six inches stroke. In the second place, if we place the value of the henry or other unit very far from its systematic value, we in many cases simply hunt the extra figures from one part of a calculation to another. For instance, if we decide to call the present microfarad, the farad, because it is a convenient size, when we wish to express the charge at a given potential, or the potential at a given charge, of a system measured with this farad, those six disagreeable figures crop out again either as decimals or whole numbers.

THE SECRETARY:—I have received the following communication upon this question, from Mr. George A. Hamilton who was unable to attend this meeting.

"With regard to the subject of inductance, I might say that probably more was done, and at an earlier date, in the way of measuring self-induction than would be inferred from Mr. Kennelly's remark concerning the history of the same. Certainly as early as 1883, and I believe earlier, it was common practice with us in the telegraph service, to measure the *extra current* of the different forms of duplex and quadruplex relays that were brought out. I believe also, similar measurement were made in the British Post-office service. Tests of this kind by myself were made by comparing the "kick" of the instruments under similar conditions in a balanced bridge, and also by shunting them with condensers of known capacity, and observing the reduced "kick." As regards the name for the unit, I am decidedly in favor of the "henry" with s for the plural. I incline also to 10^9 for its magnitude for the reasons given in the paper. I believe it will be found convenient in practice, and micro-henry can take care of the small values."

MR. THORBURN REID:—Mr. President, this matter of the choice of a unit for inductance is such an important one, that it ought to be discussed as fully and freely as possible before a decision is reached. This Institute can not force a unit on the electrical profession unless they want it, and it therefore behooves us to make certain that we have chosen the very best unit possible, and one which the profession will adopt; not because this Institute has approved of it, which fact would have very little weight with them, but because it supplies to them something that they want and want badly. Since then, this unit ought to be looked at from as many points of view as possible, I venture to give a few conclusions I have arrived at as a result of my experience with machinery having large inductances.

In the first place, I would suggest that the Committee define

clearly what the henry is to be the unit of. It is ordinarily called the unit of self-induction, but the term "self-induction" includes more, and less than the quantity which is to be expressed in henrys. It is a general term connoting all the phenomena due to the creation of lines of force in a coil through which a variable current is flowing. Thus we have the E. M. F. due to self-induction, the lag due to self-induction, the work due to self-induction-the coefficient of self-induction, etc., etc., any one of which the henry might be the unit of, and of only one of them is it the unit; namely the coefficient of self-induction. But the definition of the henry as the unit of the coefficient of self-induction is objectionable for a reason, which though technical, appears just as cogent. A coefficient is a number, a multiplier, and can have no unit except unity. A coefficient is not a quantity that can be measured or weighed, it is simply a ratio. All things considered it appears best to call the henry "The Unit of Inductance," a term suggested I believe by Fleming and used by Mr Kennelly in his paper. In the next place it would be well to define exactly what this term "inductance" means. Fleming, using the ordinary term L to denote inductance, has defined it in three ways by means of the following equations:

$$L_1 = \frac{e}{\frac{di}{dt}} \ (1) \quad L_2 = \frac{N}{i} \ (2) \quad L_3 = \frac{2\,T}{i^2} \ (3)$$

In equation (1) e is the E. M. F. of S—I. $\frac{di}{dt}$ is the time rate of change of the current. In (2) N is the total induction through the coil, and in (3) T is the energy of self-induction. The second of these equations is inconsistent with the other two and is manifestly wrong. Thus the first of the three equations gives L as the ratio of the E. M. F. of self-induction to the time rate of change of the current; in other words the E. M. F. of self-induction equals L times the time rate of change of current. The counter E. M. F. of self-induction equals the time rate of change of the total induction through the coil, times the number of turns in the coil. That is, it equals $s\frac{dN}{dt}$ but $\frac{dN}{dt} = 4\pi\mu S\frac{A}{l}\frac{di}{dt}$ $\therefore e = 4\pi\mu S^2\frac{A}{l}\frac{di}{dt}$

and

$L = 4\pi\mu S^2\frac{A}{l}$. also $N = 4\pi\mu S\frac{A}{l}i$ and $\therefore$ from equation (2)

$L_2 = 4\pi\mu s\frac{A}{l}$ { when μ—permeability, S—turns in the coil, A—cross section and l—length of magnetic circuit.

It will be found by going through the analysis by which equation (3) was found, that L has the same value in that, that it had in (1) It appears then that from (1) and (3) L will contain the square of the number of turns in the coil, while in equation (2) the turns enter as a first power. I would suggest that the Committee

define inductance according to the first and third of the definitions given above.

Another difficulty which many of us have found in this whole subject is to conceive of the inductance as a length. This conception may be of use to the mathematician, but to the ordinary practical man for whom this unit is supposed to be adopted, it is meaningless. A very good physical idea of the meaning of the term may be gained from the first of the above three algebraical definitions. By making $\frac{di}{dt}$ in that equation unity, we find that L is the E. M. F. of self-induction when the current in the coil is changing at the rate of one unit per second. This would give us the inductance as a counter E. M. F. and is tangible. Suppose, for instance, a coil of S turns to be wound so as to enclose a magnetic circuit of permeability μ and length l and cross section A. Then the number of lines that will thread the coil, when a current c flows through it, will be $4\pi Sc\frac{A}{l}$ and if the current is changing at the rate of one ampere per second, the number of lines threading the coil will be changed by an amount equal to $\frac{4\pi}{10}S^2\mu\frac{A}{l}$ in absolute units of E. M. F. To obtain this E. M. F. in volts, we must divide by 10^8, and we have $L=\frac{12.5664}{10^9}s^2\mu\frac{A}{l}$ if l and A are expressed in centimeters, and $L=\frac{31.9183}{10^9}s^2\mu\frac{A}{l}$ if l and A are expressed in inches. This appears to me to be the simplest conception of the inductance and furnishes a simple and easily understood formula for obtaining its value. These equations give the value of L in henrys. There is another consideration in favor of adopting as the unit of inductance 10^9 the absolute unit, which I have not heard mentioned, and that is the fact that, in equations involving inductance along with other electrical quantities, we may perform directly all the algebraical operations without having to change the value of L. This may probably be most forcibly brought out by means of an example. Take for instance, the equation for an alternating current in the coils of a magnet upon which a known E. M. F. has been impressed $C=\frac{E}{\sqrt{R^2+4\pi^2L^2n^2}}$ where n is the number of periods per second, E the applied E. M. F. and R the ohmic resistance. If E is in volts, R in ohms and L in henrys, we may add together directly the two terms under the radical sign and divide E by the square root of their sum and obtain the current directly in amperes. The expression in the denominator of that equation, sometimes called the "impedance," is continually turning up in alternating current work, and it will be a great convenience to the practical man to obtain its value so easily. The value of L

in the primary of a 10-light transformer, (Westinghouse) assuming a permeability of 800 (which is about the value of the permeability at the maximum magnetization) is about 9 henrys, which on open circuit will give according to the above formula a current of .14 amperes, which is in fact about the actual strength of the current in that transformer on open circuit. There is another consideration bearing on this subject whose effect it is hard to predict at the present time. That is, the variation in the value of the permeability of iron and steel. This variation will cause the values of the henrys obtained by means of the first and third of those three equations to be different. Taking L as the ratio of the electromotive force of self-induction to the time-rate of change of the current, we will get for this particular transformer nine henrys. Taking the third definition, half of the magnetic energy of self-induction divided by the current, we get in this particular transformer something less than one henry. Exactly what that means I have not been able to make out as yet.

MR. CROCKER:—In regard to the dimensions of the unit of inductance being a length, I did not suppose that there was any question in the mind of any one who had spent more than a month or two studying electricity, as to the fact that these dimensions are arbitrary to a great extent, and differ in each system, but we have adopted the electro-magnetic system, and we never use the electrostatic system and are not likely to use it, therefore it does give a certain importance to the dimensions of a unit, being always expressed in terms of the former system. Furthermore, the language of the report, which I presume brought forth this criticism simply states that "10^9 absolute units of inductance, usually expressed as a length equal to one earth's quadrant," which is certainly a fact, and it was given more to show why the unit was called a quadrant. However arbitrary the dimensions of the unit though, they are not absolute but will change with the system. But as we are not likely to change the system, the dimensions are not likely to change. As to the question of definition, I think that is a very important matter. In regard to the first point that Dr. Hutchinson mentioned, as to how the value of the unit will be affected by variations in the value of the ohm or volt; that will take care of itself. Ultimately, I presume, the volt will be the correct volt, and the ohm will be the correct ohm, and in that case the unit of self-induction will be the correct unit. In other words, all the practical units will gradually undergo a perfecting process, and when any one of them is improved by experimental determinations they will all improve. It is very unfortunate that it is necessary to go through that change, but I see no help for it, and I think that the unit of self-induction will be perfected and take care of itself, and follow along with the others Therefore to give it a particular value, would be to fix it in its present incorrect condition. In regard to which is the better definition, of the various ones that are given, I think the electromotive force

is certainly much easier to understand, and the counter electromotive force of self-induction is really the phenomenon that is practically important. The energy of self-induction is not usually so important as its counter electromotive force.

In regard to the importance of the unit of inductance, I believe that it will be almost as much so as resistance, and that the unit of inductance will in the future have perhaps nearly as much use. For example, in all alternate current work; the telephone, and the armatures of even the continuous current machines, the self-induction is as important as, or more important than the resistance. Therefore this unit is destined to very extensive use, perhaps equal to that of any other electrical unit, with the exception possibly, of the volt and ampere.

The question is exactly what the unit applies to. The coefficient of self-induction of course is the ordinary idea. But that is defined in terms of a unit change of current in a unit time. If we have ten units change of current in a unit time, then we have ten times the self-induction, but the *coefficient* will be no greater. If we have a certain electro-magnet and discharge it, and cause ten amperes of current to cease flowing in one second, the actual self-induction would be that due to the change of current, ten amperes in one second The coefficient of self-induction would be calculated from a change of current of one unit in one second. Therefore you might say that you have the *coefficient* of self-induction and the *actual* self-induction in any given case. It is perfectly possible that you might have a variation of a tenth of an ampere in a second, but the coefficient of self-induction would be that due to a unit change of current in a unit time. I think that distinction between the coefficient, which is a *special* case due to a unit variation of current in a unit time, is one thing, and the actual self induction in any given case, which may be ten units of change or a tenth of a unit, is quite another thing. The actual, is usually more important than the unit coefficient of self-induction because the former is what we really have Of course the actual self-induction is obtained by multiplying the coefficient of self-induction by the actual time rate of change of current that takes place in any given case. In regard to whether Henry is entitled to the credit of the discovery, which Dr. Hutchinson questions, I would say that the reference of Mr. Lockwood in his paper last May, was evidently to a discovery which was very vague indeed; whereas if you look back to Henry's publications which I have done, you will see that he distinctly observed the phenomenon of self-induction, and distinctly states the fact that the action was one of the current on itself, showing that he not only observed the phenomenon, but knew what it was due to, and explained it, and practically named it. That is certainly when the actual discovery took place. The mere observation of a phenomenon that is not understood, is not a scientific discovery.

next International Electrical Congress. Whether that Congress will endorse this unit is another matter, but if it has been recommended by a body of practical engineers, if it has already received sanction by custom and usage, the argument in its favor, provided that the selection has been good, will indeed be a most cogent one.

A very important matter is the question of how that unit shall be defined. If we had no iron, it would be very simple, because all these three definitions of induction—the definition by which we refer it to back electromotive force, the definition by which we refer it to the total number of lines of force through the circuit and the definition which defines it in terms of the energy accumulated in the circuit through induction, would all be one and the same thing in fact, if not in terms. But unfortunately in dealing with iron, we come across a difficulty. Mr. Crocker has said that he thinks the definition with regard to back electromotive force is the one which conveys most clearly and most effectually the nature of inductance. I have the honor to differ from Mr. Crocker in this belief. When I see a coil, I can imagine the number of linkages with that coil, and more readily estimate in this way its inductance, than I could by forming a conception of its counter electro-motive force with change of current. That is however only a matter of opinion. But there is this advantage, that the method of referring inductance to the number of lines linked with a coil is one which is very fundamental and simple. It does not require any recourse to the differential calculus, as the others do, because the time-rate of change has to be defined as the instantaneous timerate and not the time-rate in any actual period elapsed. You must define the back electromotive force as at the instant and not in any definite thousandth of a second, or hundredth of a second that has passed by. You have therefore a language which savors of the differential calculus, and it is surely important, that all our definitions should be of the simplest possible nature when we are dealing with terms to be employed practically. Apart from that, it is essential that the definition should embrace all those differences which are met with in practical measurements. If you take a coil with iron in it, and you measure its inductance with a very feeble current you will get a comparatively large inductance, because the permeability of the iron at that moment is high, and if you measure it with a secohmmeter the inductance you will get will be a comparatively large one, because you measure indirectly the total effect of passing those lines in and out, and the back electromotive force of that on the circuit. But when you come to use a strong current you will get relatively a smaller inductance because you are saturating the iron up, and you are not getting in proportion to the current anything like the same number of lines in and out of the coil, and the result is that the apparent inductance of the coil will be less. It does not matter which definition you employ, it

will not help you. It does not help you if you express it in the first definition, or the second or third, you will be hopelessly entangled and the results will be meaningless unless you specify the conditions of the test; the measurement will even differ with the particular form of apparatus employed, etc. The only hope, as it appears to me, is to specify that inductance shall be expressed by any of those three definitions, and then to say that the inductance with iron has to be referred to the inductance that would exist without the iron by the use either of the specified conditions of the test, or of the specified conditions of the iron. If you have all the conditions of the test, or if you have all the conditions of the iron, you once more bring the inductances you have measured, into definite values and quantities which will have value to express to others or to record. It would be unfair to define the inductance in one of those ways, and leave the others out, seeing that in the absence of iron all three are equally true, simply because iron distorts the phenomenon I think it would be a more wide and general acceptation to admit the three, employing the second perhaps for the sake of its simplicity, and simply say that if you use iron you must specify all the conditions, in order to explain your meaning.

I hope also that some discussion will be held, and some views promoted upon a brief symbolic representation of the unit. I think that there may be objections tothe particular letters suggested in the paper, H and h, but if anybody will give some simple means of writing henry or millihenry, it will be of considerable advantage to us. Of course it may be necessary to write the word in full, but brevity is the soul of wit and many other things, and if we can have a simple sign for it, it will help us.

Mr. C. O. Mailloux:—The remarks of Mr Crocker in reference to expressing inductance in terms of "counter" E. M. F., constitute in my opinion, a suggestion of some importance. In practice, with alternating currents, we do not deal with the instantaneous values of current or E. M. F., but we make our calculations on the basis of mean, effective, or average values corresponding to a given cycle or "period" of the electrical action. The great practical convenience of using average values for currents or E. M. F. suggests that we might profitably extend the principle further. The first method of expressing inductance ($e=L\frac{di}{dt}$) is of no practical use when there is iron in the magnetic circuit, since the inductance coeffcient L has a series of instantaneous varying values. I believe it would be of great practical convenience, and I do not think it is impossible, to arrive at a value for the inductance which would be, so to speak, "mean effective" in a sense corresponding to what is signified by the same term in the steam engineering phrase "mean effective pressure." It is not the successive stages or phases of the inductance that enter into our practical measurements and calcula-

tions, but rather the mean or average inductance for the cycle of the current (The word cycle is here used as equivalent to a half-period, since the reversal of the magnetic polarity merely repeats the same cycle so far as inductance is concerned.)

Inductance has been defined as the product of a length by a permeability, with the assumption that the permeability is unity. Now I can conceive that we might express the inductance as the product of two factors, one of which is a coefficient simply, whether we regard it as representing *length* or anything else,—(we might if we prefer call it a *length* factor or coefficient;) the other factor being what I might term the "mean effective permeability." Since the permeability undergoes a series of variations when the current rises from zero to the upper limit of maximum value reached in the given cycle, we have a series of values to consider. By integrating the curve of the permeability, plotted with respect to the current (preferably) or with respect to the time (which would amount to the same thing) we can arrive at the mean effective value of the permeability for the cycle of current (or time) passed through. If we multiply this mean value by the length factor we have the mean effective value of the inductance. Now I submit that what we usually get, when we measure inductance, is in reality the product of those two factors. Unfortunately we do not have the means of separating them, unless we regard the length coefficient as the inductance value that would be obtained if the iron were removed from the magnetic circuit; and regard the other factor (the mean effective permeability) as the factor indicating how many times the inductance is increased when the iron is present. We know, as Mr. Kennelly has just incidentally stated, that when we confine ourselves to low currents, we obtain, by measurement, a higher value for the inductance. A reference to the curve just referred to, of permeability as a function of the current will explain this readily, because the factor I call the mean effective permeability, has under these circumstances. a greater (mean) value; whereas when the iron is carried, at each cycle, to a higher degree of saturation, by using stronger currents in the measurements, the mean value of the inductance will be much smaller. I do not attach so much importance to our being able to separate the two (constituent) factors of the mean effective inductance. The principal thing needed, is a value for the inductance which can be treated as if it were constant for a whole cycle, or a whole period, and which will reconcile theoretical ideas with practical requirements. I see no difficulty in this way of approaching the question. I think it clears some points, and it may be useful as a step toward defining the kind of inductance which we actually deal with in practice; for as I have intimated, our ordinary methods of measuring inductance give us a mean effective value which we then treat as if it were constant. This mean value must of necessity vary according to

the limits between which the permeability varies under the conditions of measurement.

MR. REID:—Some points have been raised since I made my remarks a little while ago which I would like to go into a little further, and Mr. Mailloux has just spoken of one of them—what he calls a mean inductance obtained by getting a mean value of μ. It might be done if we could start from a zero current and go up to the maximum current and stop there, but if we go through a cycle of operations, the permeability varies all the way from minus infinity to plus infinity, without any possibility of getting a mean value, and the inductance, taking it from the first definition, that is the ratio of the counter electromotive force of self-induction to the current, varies through a wide range, and how you are going to get a mean effective inductance from that, I confess I can not see.

Mr. Kennelly mentioned too, that in the case of the transformer that I gave, where the inductance was ten henrys in one case and one henry in another. I understood him to say that there must have been one particular value of the inductance for that transformer. Now there were an infinite number of values. The permeability varies all the time from zero current up to maximum current, and the value of the inductance that I happened to work out, taking the permeability of the iron at maximum magnetization happened to give the correct current for that transformer when I applied it in the formula for the current in the alternating current magnet. In the other case I took the work done in hysteresis in the transformer and with known quantities worked out the inductance from that, taking the hysteresis as the magnetic work that was done in the transformer. That was the way that I got the two values of the inductance, and as I said I do not exactly see yet why those values are different, but it is evidently due to the hysteresis and to these varying values of μ. an average, which I do not see any way of getting at. Then in regard to the second definition of Fleming's, if I understood Mr. Kennelly again, he said that that agreed with his idea of linkages—the number of linkages in a circuit. If that was what he said, I would say that second definition of Fleming was N, the total induction, divided by the current, and in Mr. Kennelly's linkages the total induction was multiplied by the number of turns which is the proper value of the inductance. That is, he took the number of lines of force, times the number of turns through which they went, which is the proper value of the inductance. But Fleming's definition does not bring in the number of turns at all, except in-so-far as the number of turns determines with the current the number of lines that will go through the magnetic circuit

DR. HUTCHINSON:—I would like to ask Mr. Reid how the value of the permeability passes through infinity. I think the value of total induction referred to by Mr. Kennelly and also Dr.

Fleming's "N" is the number of lines through the iron, multiplied by the number of turns. Mr. Crocker says he does not think it proper to give any definite value to the unit. That is to say, he wishes the name to apply to some value, but does not wish to tie it down to any numerical value, but wishes it to be a floating value, coincident with the current value of the legal ohm at any time *i. e.* the secohm. But this all seems to me indefinite and vague, and contradictory to the Committee's report. Speaking of Henry, it certainly is the case that Henry published his first paper in 1832, and did nothing further until after Faraday published his very exhaustive researches on the subject.

MR. MAILLOUX:—I would like to make an explanation. I am well aware of course that a point moving back and forth in the same path would not enclose a surface, and that there could be no integration. I want to call attention to the fact that the motion one way is to be considered separately from that the other way, and that it may besides follow quite a different path. In the case of continuous currents for instance, we have the self-induction of current rise when the circuit is closed; and the inductance of current fall, when the circuit is opened, which latter may not take place until an indefinite time afterward. With alternating currents, the rise and fall follow each other closely. We must not confound the two effects however. We must consider them separately. If they are both alike, we may regard one as the repetition of the other. In either case we can perform the integration and therefore obtain the average value. I merely presented this view as a suggestion which might lead others to evolve a method possibly more correct in details.

MR. CROCKER:—In regard to what Dr. Hutchinson says as to value, I think it would be a great mistake to arbitrarily define the henry in terms of legal volts and ohms or whatever may be involved in the definition at the present time, making it necessary to change that definition at some future date, and thus bring about the infinite annoyance caused by a new definition every little while. In other words I think we better let the value be uniform with that of the legal volt, ohm, etc., at that particular time, either as adopted internationally, or as given by the best authorities. For example we now use a "true ohm" and a "legal ohm." All that would be necessary would be to specify in which of those units your henry is defined. That would be better than fixing an arbitrary value which would subsequently require considerable effort to change. The discrepancy is very small after all, and in a thing like self-induction, that varies so enormously, and where the exact values are not so particularly important, a slight discrepancy would not amount to very much, for the present. If anyone chooses to define the henry in terms of the B. A. ohm, the true ohm, or the legal ohm, it would be perfectly proper to do so, and as a matter of fact it would be exactly what is done every day by the best authorities, in the case of the ohm.

DR. HUTCHINSON:—I see Mr. Crocker would define it by the equation $rC+L\frac{dc}{dt}=E$, thus making it the secohm, and contradicting the "report" as I said before.

MR. MAILLOUX:—I would like to supplement the remark which I last made as further elucidating the view and the criticism of Mr. Reid. In our method of calculating inductance and measuring it. we of necessity assume that the inductance during the rise of the current is the same as that during the fall of the current; but there are many considerations which now come to my mind, such as that of hysteresis, that make me believe they are not the same; that they are probably different values. Of course we have no way of dissociating them, so that in reality it is the average of those two values that we deal with. This further explains why we have to take the two portions of the cycle separately, in order to establish the correct method.

[The equation is obtained as follows: The mean value of the permeability during the rise of the current is $\mu=\frac{1}{C}\int_0^C \mu dc$ and that obtained during the fall of current is:

$$\mu_2=\frac{1}{C}\int_C^0 \mu dc$$

Since the reaction is always in opposition to the current, viz: to *prevent change* in it, we can add the two effects without regard to sign, and take their average, $\mu_3=\frac{\mu_1+\mu_2}{2}$, as the average value for the whole cycle. If the inductance were the same during both phases (rise and fall of current) then would $\mu_3=\mu_1=\mu_2$. The mean effective permeability μ_3 multiplied by the length coefficient (K) will give the mean effective inductance or $\mu_3 K = L$.]

MR. REID:—I did not intend to criticise Mr. Mailloux's proposition when I made the statement about the permeability, except to say that I did not see how it was to be carried out. and the whole subject to me now, is in such a state of chaos, that I really do not know how we are going to get the value of inductance with transformers, or anything having alternating currents. In regard to the permeability getting to the value of infinity, that can be seen simply enough in any of the hysteresis curves, permeability being the induction per square inch divided by the magnetizing force, the induction remains at a finite value when the magnetizing force is zero, and it varies from plus to minus infinity in that way. The whole subject of inductance which I thought I had grasped some months ago, or may be a year or two ago, since I have been trying to work it out, using hysteresis curves and some of Professor Ryan's results, has gotten to be mixed up in my mind, and I have not yet been able to clear it up

so as to get a good clear idea of what the inductance of the transformer or any machine having alternating currents in it is.

DR. HUTCHINSON: —Mr. Reid brought forth an excellent argument against the second definition of hysteresis. According to that, the permeability of any permanent magnet is always infinity. This is of course meaningless; the definition must involve a rate of change. When I spoke, I was thinking of the definition of permeability as the tangent of the curve, which never becomes infinity.

MR. CHARLES STEINMETZ:—When you take into consideration the fact that this term inductance will be used mainly for explaining and calculating the phenomena of alternating currents, then the suggestions that Mr. Mailloux made—the introduction of this "effective value" of inductance—I suppose will be found more useful than even the true analytical term, inductance. Indeed, I can not consider the choice of the name "effective" as a happy one, because that suggests something of square-root of a mean-square, and this value, which Mr. Mailloux means, is not the square-root of the mean-square of the different values of inductance during the magnetic cycle, but it is in reality very nearly the inductance for that value, which corresponds to the maximum magnetization used, and therefore I would prefer another name, which is not so liable to be misunderstood; for instance "resulting inductance" If the current, and also the magnetism can be supposed to follow strictly the sine-law, then this "resulting inductance" is exactly that value of inductance corresponding to the maximum current and maximum magnetization used. But if the current does not follow quite exactly the law of harmonic waves, but is an ordinary alternating machine current, then this resulting inductance of Mr. Mailloux will nevertheless be so near the inductance for maximum magnetization, that with the usual instruments a difference can hardly be detected, at least not so far as my experience goes. And I had occasion to make a great number, I believe over a thousand determinations of this value of self-induction, and of the work consumed by hysteresis, over a very large range, up to values where the hysteretic loss amounted to more than a thousand watts, and I always found the value of inductance very near the value previously calculated for the maximum magnetization of the cycle, so near that the slight differences which I found—less than 5 per cent.—I can very well attribute to the errors of observation, to the inconstancy of speed of the steam-engine, etc. So I believe that when you measure the inductance by alternating currents, or for the purpose of using it for alternating apparatus, the best way will be to make use of this term of resulting inductance, corresponding to the maximum magnetization, that is the maximum point of Ewing's curve of hysteresis.

MR. KENNELLY:—Perhaps it may be safe to say that the consensus of opinion at the present time among the members, is in

favor of adopting 10^9 centimeters as the practical unit, the henry to be spelled in the plural "henrys," and that there is no difficulty in defining it in a copper coil which has no iron in it or near it, because it may be defined with equal truth in any one of three ways, those three ways giving the same result at all times, and for all currents. That so soon as iron is introduced, the inductance will probably vary with the way in which the measurement is made, the time during which the current is changing, and the value of the current, and the only way out of the difficulty from a general standpoint, is to define inductance as though there was no such thing as iron, and then to say that when you are measuring any inductance which is iron-clad, that you must specify the current that you use, and the way that you obtain it, so as to arrive at the permeability of the iron at that moment, and its rate of change. If that is done we should have no difficulty in understanding what we mean by inductance, and we should only have difficulty when one fails to give data as to the change of the currents and the permeability of the iron.

As regards the remarks of Mr. Steinmetz, which are very useful, there seem to be in different transformers certain values which must be taken as effective values of inductance. But that will depend on the saturation to which the iron is worked, and I think that if some transformers are worked with a low induction density, and others with a high induction density, that perhaps the same rule would not apply so well, and I would like to ask Mr. Steinmetz what his experience has been in this respect But in any case I think the definition of effective inductance would naturally follow but should not enter into the general definition of the term because inductance applies to all electrical circuits, of which circuits conveying alternating currents are a class

MR. STEINMETZ:—I suppose I have been misunderstood here. This term effective, or resulting inductance, is in my mind a function of the density at maximum magnetization, and I considered and used it as a function of the field intensity, in determining the permeability of the iron for the maximum magnetization in each set of readings, and using this value of permeability for my calculations. Furthermore it was not a transformer, but large electro-magnets I had to experiment with. I may add that the range of magnetization was from very low magnetization up to a density of 70,000 lines per square inch.

MR. KENNELLY:—And the same result?

MR. STEINMETZ:—Yes.

MR. KENNELLY:—Mr. President, not being a member of the Committee, I think I may safely venture to move the adoption of the report which has been presented. I think that the matter has now been well considered, and by adopting it we shall at least arrive at some definite conclusion in the matter. I beg to make that motion.

[The motion was seconded.]

THE PRESIDENT:—The Chair understands that we are already talking to a motion to adopt the report of this Committee. The report was before us on a motion to adopt at a previous meeting, and was laid on the table for consideration at this meeting. It is that motion to adopt, that we are talking to at the present time.

MR. KENNELLY:—In that case I beg to withdraw the motion to adopt, and second the other motion.

THE PRESIDENT:—As the Chair understands it, we adopt the report recommending the name henry for the unit of self-induction, and now if we adopt the present report it will recommend the value as 10 to the 9th power absolute units of inductance. By a previous vote of the Institute the name henry was adopted.

DR. HUTCHINSON:—Mr. Crocker just said it was not 10 to the 9th. He said it was something to fit the value of the legal volt and ohm.

MR. CROCKER:—In regard to defining the henry in terms of any particular equation, that has been pretty well shown to be a pretty dangerous thing to do. We have three equations, and they have discrepancies between them under different conditions, and as soon as iron is introduced, it complicates the matter very much. Therefore the defining in terms of any particular equation, would be more objectionable than to give it a value arbitrarily, because we may find to-morrow some equation that would be better or different, and I do not see any reason for selecting any one of those equations.

THE PRESIDENT:—If the Chair may be allowed to make one remark before putting the question, the Chair understands it in just this way: We are not talking about a legal henry, but we are talking about a practical unit of inductance in the same way that we talk about a practical unit of resistance. The absolute unit of resistance is defined in terms we very well know. The practical unit of resistance is the ohm, and the ohm is defined to be so many absolute units of resistance. Then we have our legal ohm which is referred to some physical standard—some quanity. Now in the same way we have our absolute unit of inductance, and we are simply passing on this report, as the Chair understands it, that the "henry" shall be 10 to the 9th power absolute units of inductance. The practical unit, the henry, shall be that. Now if you want to adopt a legal henry, that is something that is to come later. We are not adopting a legal henry to-night. It is simply the practical unit, in the same way as we define the practical unit of resistance, of electromotive force, or current, without assigning any positively known value to it in terms of the resistance of a given wire, or a given quantity of mercury, or anything of that kind. It is so many absolute units of resistance.

MR. REID:—If I may be allowed to speak on this question—a great many objections have been raised to that report to-night, and I should think that the Committee would have gotten some new ideas, and if the report is to be adopted as it stands, I do

not think it would be the best report to adopt. If we are simply going to make the henry 10 to the 9th power times the absolute unit of inductance, that we are all agreed upon. But as to the report, it might be changed somewhat. Some of the ideas given to-night could be incorporated in it, and at least the phraseology improved—I do not mean the English, but I mean that some of the technical terms could be changed slightly.

THE PRESIDENT:—The question is on the adoption of the report.

DR. HUTCHINSON:—It seems your explanation is totally different from that of the Chairman of the Committee. The Chairman of the Committee explains one thing, and you say another. It is all mixed up. I think the thing ought to be entirely gone over again and some agreement got at between the explanations of the Chairman and his Committee's report.

THE SECRETARY:—I move that the original resolutions with the amendments and the suggestions of the Committee, be referred back to the Committee, with instructions that a new series of resolutions be prepared and submitted, embodying the whole matter.

[The motion was seconded by Dr. Hutchinson.]

DR. WHEELER:—Does that motion involve sending the matter back to the Committee, to be considered another month?

THE PRESIDENT:—That is the understanding of the Chair—that the Committee will have no opportunity to report for another month.

DR. WHEELER:—If that is so, I would say that we have enough gentlemen here to-night that are sufficiently clear-headed to comprehend what we want to put through, and act on it to-night. I would like to call attention to the fact that the American Society of Civil Engineers have a Committee who have made a written recommendation on the subject which will be taken up at the annual convention to-morrow, and will undoubtedly be passed, and if we are not able to pull ourselves together to-night after two months consideration, enough to act on this simple report of the Committee, we will have other engineering societies taking the lead of us in our own department. I am a member of the committee of the civil engineers on that subject.

THE PRESIDENT:—The motion is to refer the matter back to he Committee for further consideration and report.

[The question was put, and the Chair announced that it was lost.]

THE PRESIDENT:—The question now is on the adoption of the report as it stands.

[The question was put, and the Chair announced the adoption of the report.]

THE PRESIDENT:—The next matter to come before us is, I believe the report of the Committee on the Standard Wiring Table.

[A motion to adjourn was carried.]

A paper read at the 54th Meeting of the American Institute of Electrical Engineers, New York, February 24th, 1891, President W. A. Anthony in the Chair.

REPORT OF HIGH-SPEED ELECTRIC RAILWAY WORK.

BY O. T. CROSBY.

The experiments here reported were the first serious efforts ever made to at once double railway speeds, and as such should, I think, be systematically recorded. They were made for the Electro-Automatic Railway Co. of Baltimore, Md. This company was organized about four years ago by Mr. David G. Weems, who, though not an engineer, was convinced that the electric motor would give very high speeds for the transportation of parcels. His persevering enthusiasm resulted in inspiring a number of gentlemen to subscribe money for the venture.

Mr. Weems first contemplated having an entirely automatic service from city to city, the cars to be smaller than would be necessary for placing a man.

For purpose of demonstration, a circular track nearly two miles in circumference, was laid at Laurel, Md.; gauge 28 in; weight of rail 16 lbs.; shape T; cross-ties 3x4 in. square, 16 ins. apart. Wooden stringers 3x4 ins. were laid outside each rail, serving as guards. Mud-sills 3x4 ins. were laid under the ties, and to the ties was built up a vertical frame-work on both sides of track, this carrying from cross-pieces a 2x6 in. wooden stringer, suspended above the middle of the track, while to the lower surface of the stringer was attached a small T rail, head down. This upper rail was intended to serve as an electric conductor, and also, in connection with upward pressing wheels, as a guide. The latter function was not successfully performed, the lateral oscillation of the car, on so slight a track, being too great. Part of the track was on trestle-work crossing a swamp, the overflow of which caused frequent wash-outs.

The locomotive was very simple. Three axles, carried 28 in. wheels; on these were hung a steel box, 16 feet long x 24 ins. high x 30 ins. wide. A slight vertical play of the box was provided for by springs, but, horizontally, the three axles were in rigid connection. On each of these axles it was designed to place a motor. The head was pyramidal; the weight of the car with three motors was about three tons. A second similar box of steel was made to be drawn by the motor-car, the two to be connnected by a ball-and-socket arrangement, the interval between cars to be flanged over so that only one surface should be presented to meet atmospheric resistance. The tail was pyramidal. The second car was not in use more than once.

The track-work and cars were completed as above outlined when the matter was brought to the Sprague Electric Railway &

ELECTRO-AUTOMATIC RAILWAY, LAUREL, MD.
[By courtesy of *Scribner's Monthly*.]

Motor Company, with the request that they design motors of which the armatures should be placed directly on the three driving axles of the car, the construction of which was already nearly finished. It was further conditioned that the motors should do 20 horse power of work, each, at a normal speed of about 3000 revolutions per minute. This corresponds to a car-speed of about 250 miles per hour. Needless to say, that on so light a track, none of which was straight, and with a car weighing three tons, and with only 60 horse power available for work, no such speed was attained. The motors were carefully designed by Mr. H F. Parshall, of the Sprague Company, for a normal impressed E. M. F. of 500 volts, and thoroughly filled the specifications. They were to be connected in multiple across the 500-volt circuit supplied with current from a dynamo made by the Edison Ma-

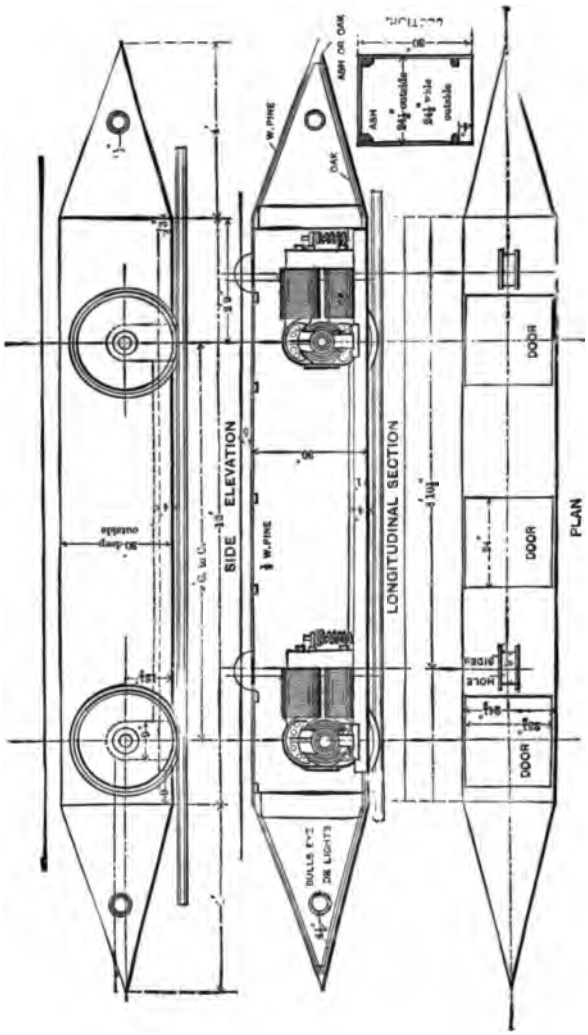
chine Works. When the motors were finally mounted and ready for action, it was found almost impossible to start the car, due probably to stiff bearings, and when several trials of speed had been made, the results were disappointing.

Up to this time the electrical work had received only such engineering attention as could from time to time be given by employees of the Sprague company, much occupied in other ways. It became evident to the Baltimore company that any satisfactory accomplishment could be had only through the continuous attention of some one having had electrical experience.

As Superintendent of the Sprague company, I had been familiar with the manufacture of the motors, and had moreover visited the plant; hence in October, 1889, it was arranged that further experiments should be under my direction. Throughout the work Mr. B. J. Dashiell, Jr., M. E. and Mr H. F. Purdy of the Sprague Electric company were of constant assistance both in conception and execution of all that was done.

Work having been resumed, it was soon found that a given speed could be attained with less current if two motors were in service than with three, and consequently the motor on the middle axle was removed. It seeems probable that, when running, this middle motor was largely occupied in skidding the wheels driven by it, since the two outer axles, on a yielding track, would doubtless support the greater part of the weight. In any case, the two motors were able to produce a greater speed than the track could stand. These machines were of the U type. The armature was wound with 100 sections of No. 12 B. W. G. wire, each section of two turns. This armature, with its commutator was rigidly mounted on the axle. The pole-pieces were attached to brackets centered on the axle, the keeper was spring-supported on a cross-piece carried by the steel casing. Bearings were of phosphor-bronze and lubrication by oil from an ordinary cup. No trouble on this score was experienced. Each magnet core was wound with 120 turns of No. 4 B. W. G. wire. At 30 amperes the armature induction was about 80,000 lines per square inch. Outline of the motor is shown in Fig. 1. These motors, since wound, according to specification, for the 500 volt circuit, were originally connected in multiple arc. This connection was changed, they being placed in series, thus diminishing the potential-control, required at the station, by reason of the fact that the machines were run at a speed so much below that for which they had been calculated.

To supply the current, contact was made between the upper rail and brushes of sheet copper set against the rail by springs. These served their purpose well. The return circuit was through the wheels and rails, the steel casing being insulated from the



PLAN
FIG 1.

axle by fibre plates and washers at joints and bolts. Rail bonds (covering joints in upper and lower rails) were used throughout; the resistance of the external circuit from the feeder junction to a point diametrically opposite was about 0.25 ohm.

The station was placed inside the circle, about 200 feet from

the track. The generator was a 70 horse power Edison machine, not compounded originally, and was driven by a 90 horse power high-speed Ball engine. At first a wire resistance of about 12 ohms was used to lower the potential on the line, as it was not convenient to run the dynamos at less than 400 volts. Later, a whisky barrel was filled with water, an iron plate, 8 inches square was placed in the bottom, a similar plate was fixed to the end of an iron rod and this rod attached to the pulley permitting easy lifting or dropping of the plate through the water. These plates being connected in the main circuit, a very serviceable liquid rheostat resulted, which, when "taken with a grain of salt," answered excellently the purpose of regulating currents which often went as high as 150 amperes.

The usual course of a run was this. Speed-observers, as many as could be had, up to five or six, were stationed around the circle, distance apart being known. Watches of these observers and of recorders who read current and potential were compared. Potential readings were taken at station and at one or two points on the line, permitting easy reduction to horse power. Mr. Dashiell, my assistant, then took the engine and I managed the dynamo. A current of from 20 to 40 amperes usually started the car. The size of the armature wires limited to about 90 amperes—45 in each machine,—the current permissible for any length of time. This value was usually attained in the first half mile of run, a considerable part of the external resistance having been cut out. When the car had attained a speed of 75 to 100 miles per hour, the line potential was in the neighborhood of 450 volts, the iron plates in the barrel being in contact, having been slowly approached as the counter E. M. F. increased. The time limitation of the runs, was invariably caused by failure of the track to serve its purpose. On three occasions the car left the track; once at 45 miles per hour, once at 80 miles per hour and once, the last time, at about 115 miles per hour. Curiously enough, on the two former occasions the derailment was to the inside of the circle, super-elevation being about four inches.

It was easy to tell in the station when a derailment was imminent, as the ammeter was most violently agitated by the make and break of the circuit caused by the particularly bad spots in the track. The agitation increased with each trip round the circle and ended in a run-off, or, after some experience had been gained, in a cessation of the trial. As the track limited the

length of run in time, so also it limited the speed obtainable. Each trial—the longest being about twenty-two minutes consecutive running—was followed by an overhauling of the track, requiring from a few hours' to a week's work by four or five men.

Could the experiments have been made on a road-bed and track, deemed even second-class according to steam railway standards of rail-weight, etc., there can be no question that without any other change, the car could have maintained for several

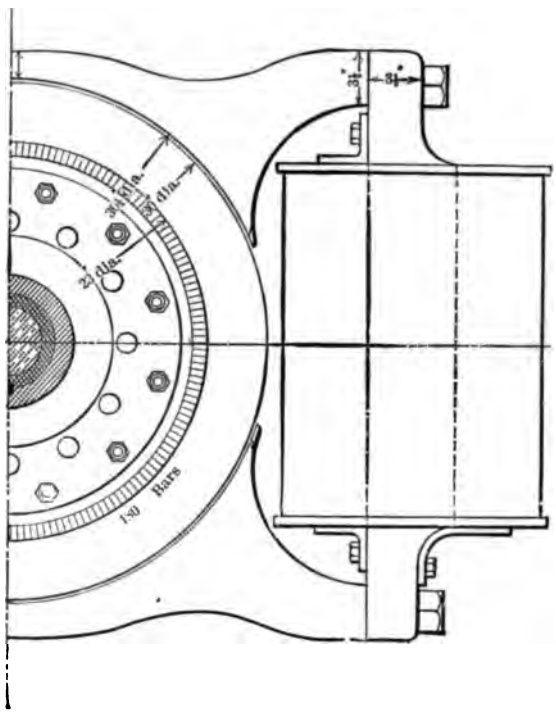


FIG. 3. Half End Elevation.

hours a speed of 120 miles per hour. Indeed, I know of no time limitation that would have arisen save that from a limited oil supply.

The speed obtained in the first round—that is in the first 9,240 feet—varied from 30 to 75 miles per hour; this determined by the management of the rheostat. The *rate* of acceleration from point to point in going round the circle was irregular, due to the grades, which going in the direction usually followed, varied from 2.0 to 2.5 per cent.

The fact that the track—always bad—went rapidly from bad

to worse during each run, made it difficult to analyze the records with a view to determining the true coefficient of traction. Moreover the coefficient for such a track, even in its best condition, would be greater than for the track that would necessarily be built for actual service. The figures obtained were of little worth as far as fixing absolute values at very high speed, but at least served to show the error of many train resistance formulæ found in the text-books. Some of these formulæ give good enough results when used for the low speeds on which they are based, but their algebraic form is such as to give rise to great error when higher speeds—say 80 to 120 M. P. H.—enter. This whole matter of train resistance has been more fully treated in a paper, "Limitations of Steam and Electricity," read by me before this Institute on May 21, 1890. The particular element of atmospheric resistance, for which special experiments were made at Laurel, was fully treated in a paper read by me before the West Point Branch of the Military Service Institute, and published in London Engineering, issues of May 31 to June 13, 1890.

Using for atmospheric resistance, figures resulting from the experiment herein referred to, we may analyze thus:

Run of October, 1889. Flat Headed Car.

Time of 1st round, 3.8 minutes. Av. speed — 2430 F. P. M. — 28 M. P H.
" " 2d " 2 8 " " — 3300 F. P. M. — 37.4 M. P H.
Average rate of work done by motors in 2d round — 15.0 H. P. 495,000 ft. lbs.
Average horizontal effort in 2d round — 495,000 ÷ 3,300 — 150 lbs.
" " " " " " for atmospheric resistance — 30 lbs.
" " " " " " " acceleration (approx.) — 10 lbs.
" " " " " " " other resistances — 110 lbs.
" " " " " " per ton — 110 ÷ 2.4 — 46 lbs.

Run of Nov. 13th, '89. Flat Head.

Time of 1st round, — 4.25 minutes. Av. speed — 2170 F. P. M. — 24.7 M. P. H.
" " 2d " — 2.5 " " — 3700 F. P. M. — 42.0 M. P. H.
" " 3rd " — 2.5 " " — 3700 F. P. M. — 42.0 M. P. M.
Av. rate of work from middle of 2d to middle of 3rd round — 15.5 H. P. — 511,500 ft lbs.
Average horizontal effort — 511,500 ÷ 3700 — 138 lbs.
" " " for atmospheric work — 36 lbs.
" " " " acceleration — 0 lbs.
" " " " other work — 102 lbs.
" " " per ton — 102 ÷ 24 — 42.5 lbs.

Run of Nov. 12th, '89. Pyramidal Headed Car.

Time of 1st round — 4.0 minutes. Av. speed — 2410 F. P. M. — 26.2 M. P. H.
" 2d " — 2.0 " " " — 4620 " — 52.4 "
" 3rd " — 2.25 " " " — 3678 " — 41.8 "
" 4th " — 2.25 " " " — " " — " "
" 5th " — " " " " — " " — " "
" 6th " — " " " " — " " — " "

Average rate of work in last four rounds — 16.0 H. P. — 528,000 ft. lbs
" horizontal effort — 528,000 ÷ 3678 — 144 lbs.
" " " for atmospheric work — 24 lbs.
" " " " acceleration — 0 lbs.
" " " " other work — 120 lbs.
" " " per ton — 120 ÷ 2.4 — 50 lbs.

Run of Dec. 3d, '89. Pyramidal Headed Car.

Time of 1st round — 4.3 minutes. Av. speed — 2070 F. P. M. — 23.5 M. P. H.
" 2d " — 1.51 " " — 6120 " — 70.0 "

Average rate of work in 2d round — 41 H. P. — 1,353,000 ft lbs.
" horizontal effort — 1,353,000 ÷ 6120 — 221 lbs.

" " " for atmospheric work — 38 lbs.
" " " " acceleration " — 42 " 80
" " " " other work 221 — 80 — 141 lbs.
" " " " ton — 141 ÷ 2.4 — 58.7 lbs.

The highest speed attained in this run was 90—95 M. P. H. track was very bad, car derailed.

Run of December, 1889. Wedge Headed Car.

Speed maintained at 100 M. P. H. with rate of work — 44 H. P. Moreover, some work of acceleration was done, but records do not permit accurate determination of this. Omit acceleration, and we have:

Average speed — 8,800 F. P. M. — 100 M. P. H.
" horizontal effort — 44 x 33,000 ÷ 8,800 — 165 lbs.
" " " for atmospheric work — 48 lbs.
" " " " other work — 117 lbs.
" " " per ton — 117 ÷ 2.4 — 48 lbs.

Highest speed attained this run — 110 to 120 M. P. H. Track better than in last run, but car was derailed at maximum speed.

These values, 46.0, 42.5, 50.0, 58.7 and 48.0 lbs., should be corrected for curvature, making them, 30.7, 28.4, 33.4, 39.1, and

32 lbs. respectively. Other and lower values were obtained when the track, as at first, was in better condition. But these earlier runs were not recorded in exact figures The rails were in all cases rusty.

The maximum speed—that of final derailment, given above, could not be absolutely determined, since the car left the track at some distance from the last two observers, whose time records gave the speed attained between the points of observation. It was clear however that the final speed was between 110 and 120 miles per hour. A length of 1000 ft. of track was injured, about 300 ft of the upper framework wrecked, and the car itself was landed about 30 ft. from the track, having finally jumped the guard rails while going over a fill 4 or 5 ft. high It seemed clear that the Laurel plant had passed its days of usefulness. Nothing further of value could be done without practical rebuilding. It was then time to make new plans, and this work was at once begun by Mr. Dashiell and myself. At the outset I felt that—if for no other than commercial reasons—we should plan for the carrying of passengers as well as parcels. This meant an increase in the size of cars and a change from an attempt at automatic control, to control by human intelligence on the locomotive.

For purposes of demonstration I proposed to the company that they should build a track about four miles (not less) in circumference and should run thereon a train of two or three cars drawn by one locomotive. Calculations were based on the following data:

(1) A speed 150 miles per hour on a level was to be aimed at.

(2) Cross-section of car should be a minimum consistent with the seating of passengers. This was taken at 6 ft. by 5 ft., 30 sq. ft. (crowning higher along middle of car.)

(3) Gauge of track should be standard—4′—8.5″

(4) Character of track should be simply the best that art could contrive, rails to be from 65 to 90 lbs.

(5) Electro-motive force should be as high as the art of insulation would permit.

(6) Whatever might be the number of cars in a train, all were to be so connected as to present a continuous exterior, thus presenting only one cross-section to the atmosphere.

(7) Atmospheric resistance at 150 miles per hour would be taken at 15 lbs. per square foot of cross-section, a wedge or par

abolic locomotive head being used. This value is 50 per cent. in excess of that indicated by our experiments.

(8) Traction coefficient, exclusive of air resistance, would be taken, at 150 miles per hour, to be 25 lbs. per ton—this for reasons set forth in previous papers.

From the two resistance coefficients just given, it followed that for every square foot of cross-section, regardless of weight,

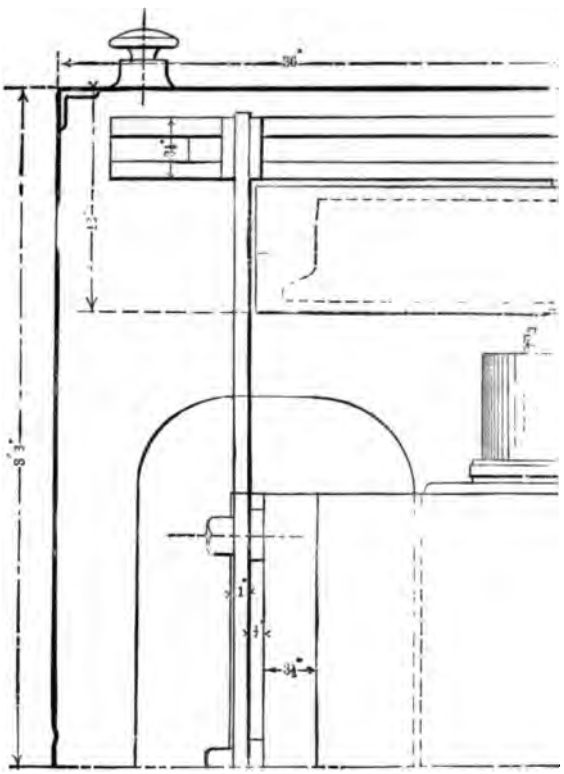


FIG. 3.

6 horse power would be required, and for every ton of weight, regardless of cross-section, 10 horse power must be supplied.

For a locomotive of about 600 horse power having 30 ft. cross-section the dead weight was calculated to be about 18 tons. Steel cars of equal cross-section were designed, weighing, empty, about 5 tons, with carrying capacity of about 5 tons.

The important question as to power required could then be thus tabulated:

	150 M. P. H.	120 M. P. H.
Locomotive alone.....	360 H. P.	288 H. P.
Locomotive and one car loaded.....	460 "	369 "
Locomotive and two cars loaded	560 "	446 "
Locomotive and three cars loaded	660 "	528 "

To perform the work here indicated two motors were provided, one armature directly on each of the two axles of the locomotive car. These motors, outlined in Figs. 2, 3, 4, 5, were of the Manchester type. They were supposed to be at first connected in arc across a 1500-volt circuit, each taking about 130 to 150 amperes and delivering about 250 to 300 horse power. Should it be desired to experiment with higher line potential, the motors in series would permit 3,000 volts to be easily tried. The armatures of the Gramme type are 30 ins. outside. 23 ins. inside diameter. The armature conductors were to be of about 40,000 circular mils, cross-section, or from 500 to 600 cm. per ampere. Many successful machines, designed to work indoors, go as low as this figure, and since the dissipation of heat must in such case proceed more slowly than in the case here considered, the figure seemed very safe. Moreover, it was intended to introduce a blast of air from the car front which should continuously flow through the open centre of the armature (Gramme type). How far the rate of dissipation would be carried by such means can not now be known, but all recent evidence (see experiments of Mr. Kennelly) goes to show that the heat capacity would be much increased.

The cross-section of core in the armature was 3.5″ x 40″ = 140 square inches gross, or, deducting 15 per cent. for paper insulation between discs, and 8 per cent. for conductor slots cut in periphery, 110 square inches net. Through this armature it was desired to force 22,000,000 lines (C. G. S. units) or 100,000 per square inch = 15,200 per square centimeter.

The hysteresis loss in such an armature revolving at 1200 R. P. M. (equivalent with 42 inch wheel to 150 M. P. H) was calculated to be about 3.000 watts—following the curve published by Esson. The $C^2 R$. loss ($C = 135$ amperes, $R = 0.26$ ohms when T 75° C) = 4740 watts,—or about 8000 watts for the two losses. I found it difficult to obtain such coefficient as would give reliable values for loss due to Foucault currents. With very thin plates it would not be large. From what precedes, it followed that the number of armature conductors should be 340. These were divided into 170 sections, wound in one

layer, each conductor to lie in a slot; both slot and conduct were planned to be rectangular, or nearly so, with the reservati

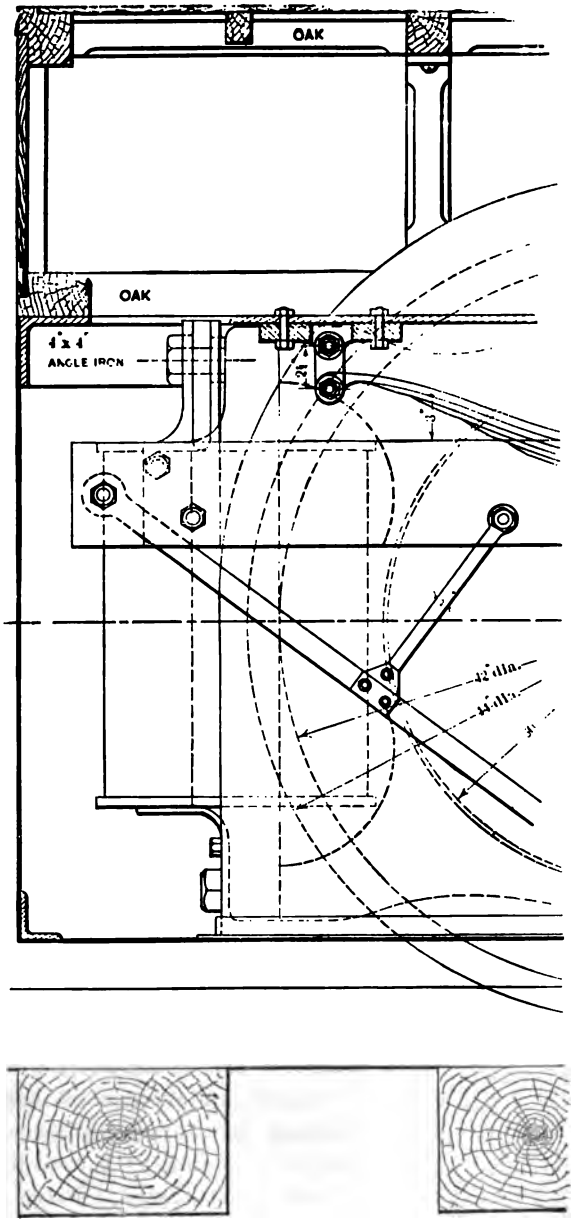
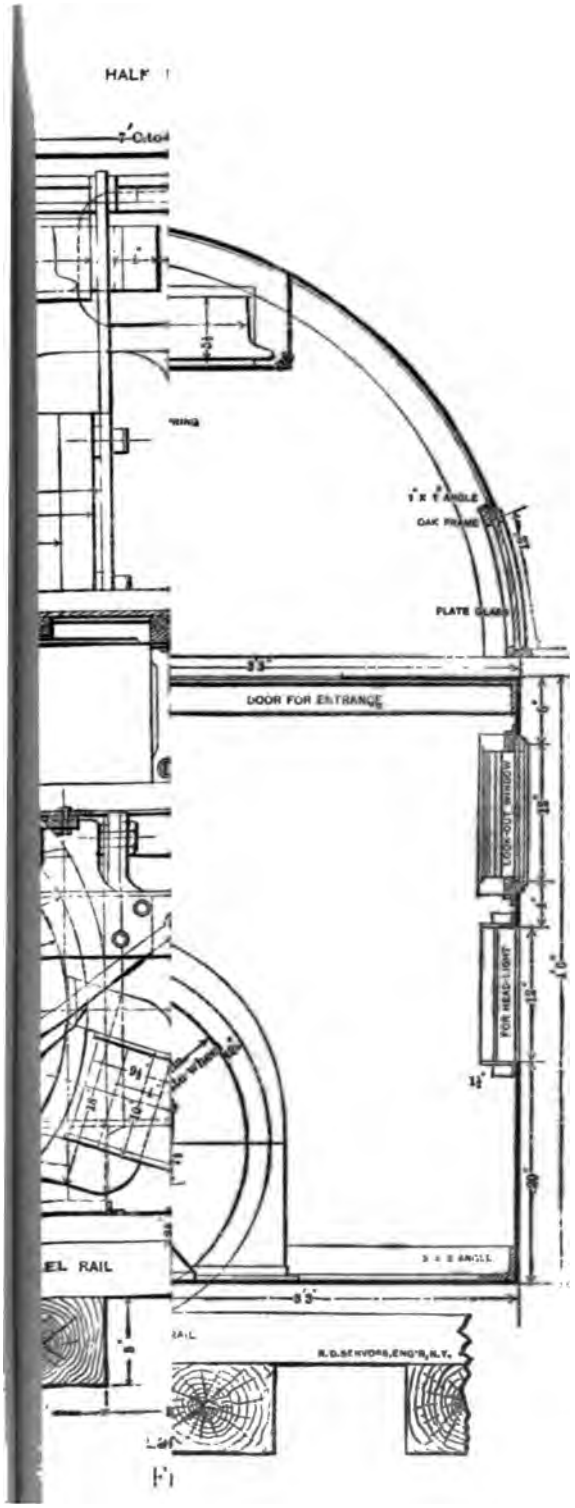
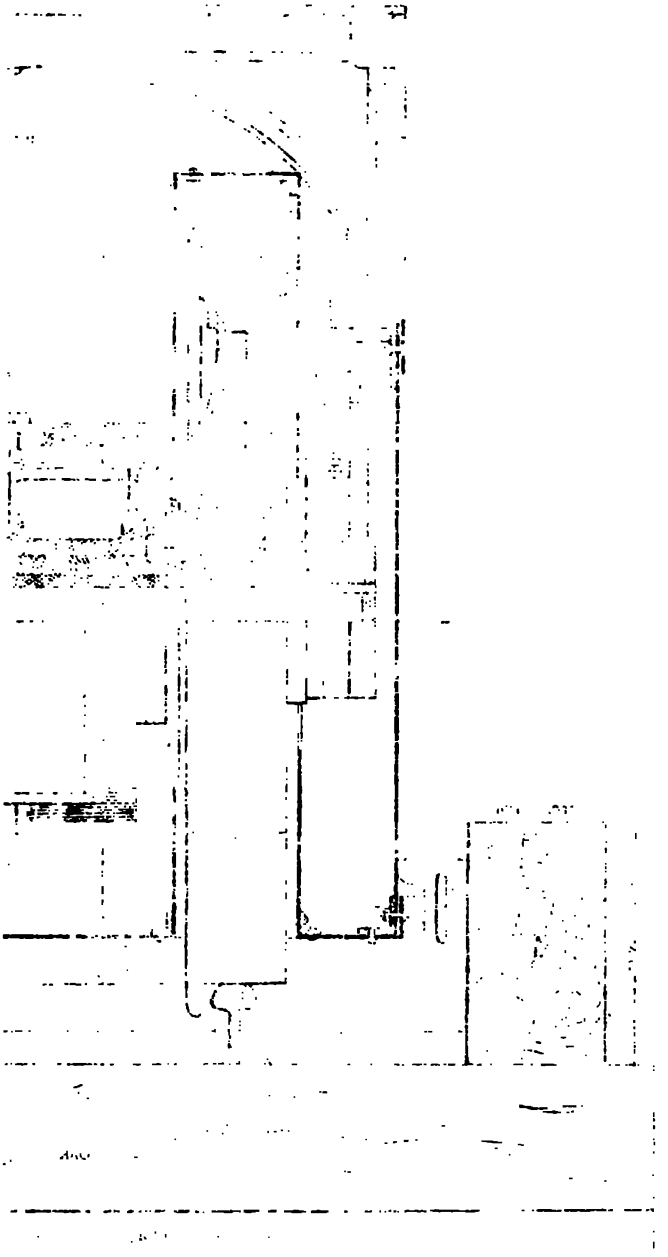


FIG. 4.

that experience in manufacturing would soon show the prop detail.


HALF
OAK FRAME
PLATE GLASS
DOOR FOR ENTRANCE
LOOK-OUT WINDOW
FOR HEAD-LIGHT
EL RAIL
R.D.SEYMOUR,ENG'R,R.T.



The magnets have a cross-section of 164 square inches through which 14,300,000 lines of force must pass, the increase from the number in the armature being determined by a leakage coefficient 1. 3.

The magnet coils of the two armatures were calculated to be in series with each other, but in shunt with respect to the two armatures. This variation from present street car practice I thought to be justified by the following considerations: First, no system of commutation could obviate the need of a considerable external resistance, which then could be easily made sufficient, within itself, for speed control; second, the maximum torque for a given armature current is always desirable and can be best obtained by permanent saturation; third, this constant and maximum magnetization would tend to diminish sparking—an evil to be specially avoided in long distance work, involving continuous runs of several hours.

While it was thus intended to use shunt coils at first, I felt that a little experience would soon positively demonstrate that the advantage lay with this or the series winding.

The commutator, Fig. 5, was designed to be 23 inches in diameter, which would of course give rise to a very high speed relative to the brushes. I believed it better to face the mechanical trouble that might be connected with great circumferential speed rather than to diminish the diameter by decrease of sections, since this in turn might produce sparking.

Without going into the mechanical detail of the commutator, I may add that a single bar could be taken out or replaced, without removal from the shaft.

The rheostat for the armature circuit was designed to be of iron wire, with the expectation however of experimenting also with liquid or carbon resistance. The resistance and heat capacity of this main line rheostat were determined with reference, first, to current necessary for starting, second, to currents needed for maintaining low speeds. The starting current on practically level track was calculated to be from 300 to 400 amperes, the load being 80,000 lbs. (or locomotive and three loaded cars.) For continuous running at 30 miles per hour, a current of 40 amperes would be required, the counter E. M F. of each of the 1500-volt motors being, at this speed, only 320 volts. The line-potential being 3,000 volts, it would be necessary to have in the armature circuit an external resistance of

59.0 ohms $= \frac{3000-640}{40}$. The wires were calculated to stand 40 amperes continuously, and the various sections were proportioned to stand various currents for such time as would serve to bring the train up to speed. It was of course known that a considerable reduction in the size of resistance box could be effected by winding the motors for the regular line potential, thus normally working them in arc, but putting the armatures in series where a great reduction in speed might be required.

A small rheostat and a condenser were designed to be placed in the circuit of the magnet coils, and a rheostat also in the lamp circuit, these resistances giving delicate control.

The problem of retardation for a mass of say 40 tons running at 150 miles per hour is a serious one. The Galton and Westinghouse tests indicate a very low value for the coefficient of friction between brake-shoe and wheel, at high speeds; they report it in some cases at 60 M. P. H. as low as 0.04. As however it rapidly increases at lower speeds, an average of 0.1 may be taken. The total retarding effort, i. e., brake resistance and track and atmospheric resistance must be kept below that which will just produce sliding of the wheels, and the coefficient of sliding may be taken on an average at 0.08 of the weight of the wheels. If then we represent by P the maximum allowable brake pressure on the wheels, by R resistances other than brake friction, and by W the weight on braked wheels. we may write the following inequality: $0.1\ P + R < 0.08\ W$.

The value of P should be determined as that which, with a small margin will satisfy this condition of inequality.

Following this calculation it was found that a brake-pressure of about 5,000 lbs. should be applied to each wheel. This was designed to be produced by magnetic brakes similar to those used by Mr. Daft on the New York Elevated Railway. The form and approximate dimension of those brakes are shown in Fig. 6.

A mass of 80,000 lbs. moving at 150 miles per hour represents 64,000,000 foot-pounds of energy. Average resistance due to brakes $= 0.1 \times 60{,}000 = 6{,}000$ lbs. Average resistance from track and atmosphere for average speed of 75 miles $= 400$ lbs.

In addition, the motors may be required to generate a current which shall be wasted over the rheostat. Since the field-circuit in the case of shunt-motors, is independent, magnetization may remain constant, and average E. M. F. of 1500-volt motors (i. e.,

motors generating 1500 volts E. M. F. at 1100 R. P. M. or, with 42″ wheels, at 150 E. M. F.) would be, while the train was coming to rest, 750 volts. Make the external resistance such, that the two armatures being in series, an average current of 200 amperes shall flow. Then average dynamo resistance, in lbs. = 2000. Total retarding effort = 6000 + 400 + 2000 = 8400 lbs. Dividing 64,000,000 by this last quantity we have 7620 ft as the length of run to come to a stop, and the time of stopping about 100 seconds.

For the mechanical construction of the locomotive, two plans were contemplated. One has a 12-foot rigid wheel base and no pilot wheels; the other has a 7-foot wheel base for the drivers and a pony-axle in front, free to move laterally over a certain distance, dragging the drivers in the same direction. This is the general principle of pilot-wheels, so largely used on high-speed engines. This second arrangement is preferred. In it nearly all the weight goes on the drivers. In this particular case, total weight of locomotive is about 18 tons, each motor being six tons. Of the total, 15 tons rests on the drivers. If then the adhesion-coefficient, while running, be taken at only 0.1, a circumferential effort of 3000 lbs. may be exerted without slipping the wheels. This is safe, since if 600 H. P. of work be required at 150 miles per hour = 132,000 F. P. M., the horizontal effort $= \frac{600 \times 33,000}{13,200} =$ 1500 lbs. For starting the load, adhesion-coefficient may be taken at .25, giving 7500 lbs. possible pull, this being much in excess of the value required.

The locomotive construction is such as to put the entire weight of the armature directly on the axle. In designing this, I did not overlook the fact that the armature must thus be subjected to much jarring. But I believe the difficulty should be met by excellent mechanical construction, rather than that spring movement should in any way be attempted. For mechanism that shall succeed in high-speed, long-time service, simplicity is a requirement of the first order. Indeed, generally, in this work of connecting an electrical motor to any vehicle, I have long felt that he who is over-ingenious in contriving many parts and fine adjustments, may not be over-successful.

The weight of the magnets and pole-pieces goes also directly on the axles, save for the intervention of rubber plates having a maximum play of 1-16 of an inch. By the use of motors of the

"U" type, part of the weight might be spring-supported, but on the other hand it is not as easy with that type, as with the Manchester, to obtain a perfectly sparkless motor. Moreover the weight going dead on the axles in the case considered, is not greatly in excess of that due to 8-foot drivers and other attachments of large steam locomotives. Further the need of any spring movement decreases with increasing perfection of track-work; as, in ideally perfect track, car-springs would not, save on curves, come into play.

In the first design of locomotive cars, the operator is to sit between the two motors, where also would be placed the controlling devices. In the second design, the operator would be placed over the pony-axle, the devices being chiefly in the cylindrical or parabolic head.

Detailed drawings of freight and passenger cars were completed, but need not here be referred to.

As to the supply of current, it is proposed to use the double metallic system. This departure from the ground return now so commonly employed in street railway work seems justified by the following considerations:

1st. For a given voltage it diminishes the cost of insulation, *per se*.

2d. It diminishes the danger of accident within the motors.

3d. It diminishes danger of accident to workmen.

4th. By being thus more easily handled it is probable that a higher voltage will be found practicable than with the ground return; hence in fact even first cost might not be greater.

As designed, the conductors will be placed in an inverted wooden trough, attached to posts placed at 12 foot intervals on each side of the track. The trough to be about 5.5 feet above the ground. The conductors for each side of the circuit to be two—one insulated and continuous; the other a bare, flat strip, broken into sections, these being normally out of circuit; the locomotive trolley-arm operating a switch which shall throw out the rear section and throw into circuit the section ahead. Such a switch was designed in detail.

As to what the line voltage should be, progress in the art of insulation can alone determine. It would of course not be necessary, on a roadway for long distance travel, to consider the death-producing voltage as a limitation.

In the important matter of limiting curvatures, calculations

were made in the ordinary way, using the particular values for weight and height of center of gravity. A safe speed was then tabulated as that which is just one half the speed that causes the resultant of weight and centrifugal force to pass through the outer rail of a curve. The beneficial effect of superelevation, neglected in the calculation, would, of course, practically increase the factor of safety.

The following table resulted from method described:

Radius in feet.	Safe speed, by rule above, in miles per hour.
1,000	71
2,000	98
3,000	120
4,000	140
5,000	158
6,000	172
7,000	187
8,000	198

Of course, naught save practical trial could determine whether the assumed factor of safety, which is quite large enough at present speeds, would in fact, be right for the higher speeds aimed at.

Commercial Aspects.

Copper calculations were made, based on a station potential of 3000, and also of 6000 volts, stations being 50 miles apart (thus supplying current 25 miles on each side) and on a service requiring one train on each 25 mile section, the drop on the line being 33% of station potential. Then for a line of 1000 miles, as from New York to Chicago, with copper at 15 cts. per lb., and for 3000 volts initial:

Cost of conducting system, including frame-work	— $7,000.00	per mile.
" station per mile, including steam	— 3,000.00	"
" double track, including depots, etc.	— 55,000.00	"
" train equipment, 3 cars each, 50 trains and engines,	— 1,000.00	"
Total per mile,	$66,000.00	

In this, the cost of track construction is based on road-bed figures reported for the Erie Railway Co. To arrive at operating expenses, suppose 20 trains each way each day, and a schedule of 125 miles per hour, *i. e.* 8 hours for the trip. Suppose a station development of 800 H. P. per train on the line, or 64,000 H. P. hours per trip. In stations of large capacity working under the supposed conditions, the cost of one H. P. hour = 0.90 cent (see tables in previous paper "Limitations of Steam and Electricity")[1] or per trip, for power a cost of $57.60. For train men, suppose

1. Transactions, Vol. vii, p. 265.

two men on each train, and one per train in reserve, and suppose $3.00 per man per trip of 8 hours, then, cost per train trip = 3 × 3 = $9.00. Interest charge per train trip =

$$\$66,000,000 \times 0.05 \div 365 \times 40 = \$225.42$$

For maintenance of way (see Wellington's Statistics,) take $1.000 per mile of double track per annum, or per day for supposed line $2.700.00 per trip or 2700 ÷ 40 = $67.50. For general expense of operating company, suppose $1,000.00 per day—or per train trip $25.00. For wear and tear, oil, etc., on train itself, take $10.00 per trip. Then total = 56.60 + 9.00 + 225.42 + 67.50 + 25.00 + 10.00 = $394.42.

For receipts:

Suppose average train be of two cars, (such as designed) having each a capacity of about 10,000 lbs. freight (considered such as express and mail matter), or of say 15 passengers. Suppose average train-load of freight, 15,000 lbs., of passengers, 20. Take freight at 33 cents per cwt., passengers at $25.00. The income per trip in either case—$500.00, showing profits, over fixed charges, of say $100.00 per trip, or $4000.00 per day. The total daily load of freight, each way, required to justify the service above treated, is 300.000 lbs., or if 200 through passengers be carried, (*i. e.* ten trains for passengers each way each day) then only 150,000 lbs. of freight. The *present* movement between New York and Chicago, in mail and express, is not far from this figure. A five-hundred mile line, connecting Boston, New York, Philadelphia, Baltimore and Washington, would be even more profitable. It is to be remembered also that a service of *small* trains is contemplated, making fixed charges relatively very large. Much larger trains could be, and I think *will be*, run at the speeds here considered, but I have supposed that the smaller effort would precede the larger. It is further to be noted that the use of 6,000 volts potential, and the extension of some existing roadway instead of the building of a new one, would diminish first cost. As grade-crossings would be out of the question, and as cities would require to be entered above or below the surface (country roads to be carried over on bridges) it is perhaps best to consider only the larger figure already used.

To demonstrate all or nearly all that has been outlined in this paper will cost about $300,000, covering the construction and operation for a reasonable time, of the four-mile circular line

proposed above. Last year the Baltimore company met unexpected difficulty in efforts to raise the needed money; their operations were suspended and my connection with them ceased just at the time when plans were completed—even to working drawings. I have been much pleased to learn, within the last few days, that far from being discouraged, they are now confidently hoping that they may push on, in the near future, the great work to which they have laid their hands.

Unfamiliar as is the project, you will perhaps be the more ready to share the confidence of its promoters, on hearing the words from Prof. Henry A. Rowland and Dr. Louis Duncan, of Johns Hopkins, to whom were submitted, last spring, full details of the plans partially described in this paper. After reviewing the figures for power required, they say:

"We believe from the data obtained that the values given are not too low, and that the horse power which Mr. Crosby calculates is *not less than the amount required.* While we have investigated carefully and considered all the data obtainable, yet the existing experiments are not sufficient to accurately fix a limit to the train resistance. *We believe, however, that the value assumed by Mr. Crosby is safe.*"

"The motors, the calculations and drawings for which are in the appended statement, will develop the horse power for which they are designed, namely 500 horse power. We point out some modifications which will be beneficial. *We believe that they will drive the train* (locomotive and two cars, O. T. C.) *at the required speed.*" (Italics here and elsewhere are mine, speed then considered 120 on a level, O. T. C.)

"The possibility of a train being derailed by an obstruction on the track increases with the speed. At speeds up to 90 miles, however, there seems no increase in the number of derailments. In the case in question, the centre of gravity of the cars is very low, and *it would be difficult to derail them on straight parts of the track.* The radius of the curves should of course be great, but not so great as would be required for an ordinary train going at these high speeds. *The question of safety is*, however, *almost wholly a question of track construction.* Considering the form of the proposed train, its comparatively light weight, making a less demand on the track, it is certain that, with a carefully constructed road, it could attain with safety, speeds which would be impossible with trains as at present constructed. As these latter have several times made 86 miles, and often made 80 miles *it would seem that a speed of* 120 *miles or even more, with the electric cars, would not be outside the limits of safety.*"

"The plan for supplying current to the motors is feasible."

"The design of the motors is discussed in the detailed report. It is generally good."

"Should it be demonstrated by an actual test that passenger trains can be run safely and economically at a speed over 100 miles per hour from here to Chicago, the financial aspects of the case would certainly be improved. *We are of the opinion that the chances are in favor of this being accomplished by the present scheme.*"

These words, though guarded and accompanied by criticism of the detail, are yet a substantial approval of what was submitted. To me, they were very encouraging.

[Pressure of other business has prevented me from carefully reviewing the calculations appearing in this paper.—O. T. C.]

DISCUSSION.[1]

THE PRESIDENT:—[Prof. W. A. Anthony.] This paper is now open for discussion, and I trust the interest which it must excite will call for remarks from many whom I see present here to-night who have had something to do with electric traction.

MR. W. H. PEIRCE:—I would like to ask Mr. Crosby if he has made any calculations or estimates as to how far and how long the train would run before it would attain a speed of 150 miles, and how far it would have to run before it can be stopped.

MR. CROSBY:—I did make calculations as to the attainment of the speed, and the matter of stopping it I expressed in the paper. I knew that those figures could not remain in the minds of those hearing the paper. The space for stopping, with a braking power that would just fail to slide the wheels would be just about 7000 feet and the time would be in the neighborhood of 100 seconds. The time of getting the train to speed, with the motors as designed I have forgotten now, but it is in the neighborhood, I think, of two or three miles.

MR. PEIRCE:—In getting an ordinary train to speed at sixty miles it will take over three miles, and I should think it would take ten or twelve to get it to a speed of 150.

MR. CROSBY:—It may have been more than three or four miles, but I think they would attain that speed with the lighter weight they have to handle, and the power proportioned to the weight, in six miles.

MR. PEIRCE:—The only reason I bring this up, is, that in running these trains a short distance it would not be possible to attain very high speed.

MR. CROSBY:—I should say that I do not think there is anything whatever in high-speed work for say, even forty miles. I do not think you can properly do 120 miles an hour for a run of say thirty or forty miles. It is applicable only to connecting very large cities. I think you could run from here to Albany, if you

1. By Messrs. Peirce, Crosby, F. L. Pope, Lockwood, Sprague, E. P. Thompson, J. S. Brown, Geyer, P. H. Dudley, Mailloux and Wirt.

had the New York Central line to run on; from Albany to Rochester, from Rochester to Buffalo, and then stopping at the principal lake cities. It might be found later on, that it would be necessary to make shorter stops; but the first efforts should be made only with very long runs.

MR. F. L. POPE:—In conversation some time ago with a prominent civil engineer, at one time professionally connected with the Lake Shore road, he expressed the opinion that safety from derailment at very high speeds would be best secured by very slightly curving the line of the road, just sufficient to cause the flanges of the wheels to bear constantly against one side. With that construction he should expect that any possible speed, up to say 200 miles an hour, that could be got from the motors, would be perfectly safe. He was led to this conclusion from long observation of the performances on the Lake Shore road of railroad wheels at high speed, on which road as many of you are aware, some of the best running in this country has been done.

MR. THOS. D. LOCKWOOD:—I have been very much impressed with the facts which have been so clearly furnished to us by Mr. Crosby, and before I forget it, I should like to add one or two remarks, with regard to what he has just now said—that he does not consider high speed, extremely high speed, or even so low speed as forty miles an hour to be a very important factor except for extremely long runs. I do not know whether I correctly understood that.

MR. CROSBY:—What I meant to say, Mr. Lockwood, was, that I did not think a speed of 120 miles an hour was commercially applicable to making short runs of say forty miles. Suppose we have to stop forty miles away from New York, I do not think it would pay even if it were a good sized city, to establish a service there, aiming at a speed of 120 miles for a very short distance. You would not more than obtain it, when you would have to give it up.

MR. THOS. D. LOCKWOOD:—That is a little different from what I understood at first, but it does not interfere at all with what I was going to say, which is this: I have had some experience in steam railroads. I have worked in various capacities upon railroads, and I have had some experience upon steam locomotives, and such experience as I have had leads me to this statement—that the best speed I have ever known to be made out of an average of a great many trips has been done by way trains where the stations were very close together; and the reason of that is, that upon a long run you can obtain a fairly high average without even getting an immensely high speed; whereas upon way trains, where the stations are very frequent we often have to run at enormously high speed between stations to maintain the time, and that is often when we have runs of only two miles between stops. There is a road I have in mind, in the western part of Massachusetts and Connecticut on which I have worked—the

Housatonic Railroad—and between Pittsfield, Mass. and Canaan, Conn., there are a large number of stations; I have known trains to leave Pittsfield some fifty or sixty minutes behind time, and reach their destination on time. Now that sounds rather fabulous to gentlemen who have not been on locomotives or who have not run locomotives. I heard statements made at meetings of this association some time ago, in which Mr. Ries took part, and it was argued then that a great part of the power saved while running down grade might be used to operate dynamos for lighting purposes. Now that is a very prominent method, but the experience of all locomotive engineers leads them to the conclusion—whatever it may lead persons who have not done that kind of work to conclude—that they want all the power all the time, and they are very glad indeed to get a chance to run down grade once in a while.

I noticed that Mr. Crosby in his paper, made no reference to what some eighteen months ago was a very prominent issue, namely, the improvement of adhesion by the passing of a current from the wheels to the rails. I recollect in different meetings, social, legislative, and litigative, in which I have had the honor to appear both as a spectator and participant, that that issue was made very particularly prominent. Some electricians ascribed it to magnetism, which in some occult way, escaped from the motor and reached the wheels and from there passed to the rail; and while I do believe that magnetism is going to be one of the great braking powers of the future I do not believe it is going to reach the rails that way. But I see that Mr. Crosby has advanced from the opinion which was, to say the least, popular for a few years. In prior meetings of this Institute, Mr. Leo Daft and others have made certain statements—I do not know whether they were deliberately written on paper or made on the spur of the moment in discussion—very strongly assuming that there was such a useful effect; Mr. Ries also was a very prominent advocate of that alleged method of improving adhesion. I do not think there can be any doubt that when heavy currents pass from wheel to rail or between any metallic contacts there is a certain slight adhesive effect. I do not think it has any practical effect upon the adhesion which a train in practice requires. In 1878 that very eminent mechanician, Mr. Stroh, in London, ascertained that in passing even comparatively feeble battery currents between contacts, that there was a very decided adhesive effect upon a small scale; and he tried this with a great number of metals and alloys, finding that steel—two contacts of steel—gave the very best effect, as compared with other metals. In the light of what we know now, it is very clear that it was a welding effect; that even small currents in passing through loose contacts developed enough heat to make a weld. Mr. Stroh used a lever with a weight upon one end, and in some of the experiments the lever had an equally long arm at the other end,

and after passing the battery current through those loose contacts and after placing a weight thereon, it was found that that equal weight would not separate the contacts again. But I am compelled to say from my own practice that I do not believe that such an adhesive effect as may exist has any practical utility; for this reason, among others; that such power as it does exert in holding the wheel on the track, is lost by the equally certain fact that it must obviously require as much force to unweld it again so as to admit of the progress of the train

MR. CROSBY:—I may answer that I did not mention the fact in the paper.

MR. LOCKWOOD:—I know you did not, and I wished to ask you, whether you had any decided opinion.

MR CROSBY:—Very decided indeed. I do not think there is anything in it at all. I have put myself on record as thinking there is nothing in it—short of such currents as will actually heat to welding. I know of experiments which were never published, but which were made by a man much interested in the subject at one of the great universities of the country, which indicate that in a three or four pound model it took something like two hundred amperes to produce any effect, and that was plainly a welding effect, since it showed a clear pitting of the little rail on which this model used to run Now the number of amperes used there—certainly one hundred—was surely out of proportion to the current that is used for any time for transportation. That perhaps is like the answer of the mayor who said he did not have any cannon and therefore did not salute his majesty—that was one of his ten reasons. In this work here described there cannot be, in my opinion, any such grade work attempted as to make necessary a distinction from that of ordinary coefficients, even if it should be obtained by the use of extravagantly high currents. I do not know what would be the steepest grade. It is simply a question of how much you are willing to sacrifice in speed in order to save original expense in constructing your line. It is the same question that is presented to the steam railway engineer. He cuts between the two extremes. I should say roughly now that perhaps a two per cent. grade would be a pretty heavy grade to contemplate for that work on account of the great tax on the motors and the actual requirement of power. Then in the matter of the braking effect I do not know whether Mr Lockwood meant the return of power from the dynamos as the braking effect which he thought would be useful or not.

MR. LOCKWOOD:—No; I meant the idea of short-circuiting. Such a scheme was patented within the past ten years. I do not believe it has ever been used.

MR. CROSBY:—I did not consider the matter of return of energy to the line, because, as Mr. Lockwood says, they want all the power they can get, and because the number of stops is very small compared with the number of miles run. It might be

different, say in the elevated railway service. It might do to run your motors as dynamos and let them give back the work of retardation into the line. But you make a little sacrifice to do that. That is you have got to work your motors at lower than saturation point for your normal duty. Otherwise I do not see how you can get the electromotive force of the motor high enough to pump back into the line. For that reason I do not think it has any place in long distance work where your stops are one in a hundred miles.

MR. PEIRCE:—I would like to ask Mr. Crosby, in working the size of wire out for the most economical conductor how much variation of the electromotive force he found in the line from the terminals to the centre.

MR. CROSBY:—I did not use the ordinary equation for determining what would be the most economical drop on the line. I simply took almost at random, as I was very much pressed to get this into shape; I took thirty-three per cent. drop on the line, and had done so all along, thinking that it would be practicable to have that; although I do not know that it would be the most economical drop, not having calculated.

MR. PEIRCE:—I meant the variation, you know. I did not mean the total drop. I meant the variation between different points of the line.

MR. CROSBY:—That is connected with the amount of drop. If you had thirty-three per cent. drop that would give you an inference at once as to all the variations there would be on the line.

MR. PEIRCE:—I did not know but what you would run out feeders so that you would have only four or five per cent. variation at different points of the line.

MR. CROSBY:—That might be desirable, but I think it would be just as well to put your whole motor in once for all instead of whittling down. Thirty-three per cent. was at the furthest point to which any station would supply the current.

MR. EDWARD P. THOMPSON:—I would like to ask if it is your opinion, based upon your experiments, that it is impracticable to run an automatic electric train for the purpose of carrying mail matter and other objects of great value where it is necessary to have high speed? It is even of more importance, it seems to me, to transport such from here to Philadelphia, Chicago and other distant places at a great rate than it is to transport people. A great many of those high-speed passenger trains were reduced in speed because it was found that people really did not care to go so fast, and because of the wear and tear on the locomotive machinery; and therefore the high-speed trains running at over sixty miles per hour were to a great extent taken off both in England and the United States as fully stated about a year ago in the reports of several railway companies; but if, in practice, they actually could get a speed of 120 or 150 miles an hour, it

would be more acceptable, I think, for small express packages and mail than it would be for passengers.

As regards brakes for stopping high-speed trains, the principal point is economy. I found in some tests made by me in behalf of a brake company upon a railway in Pennsylvania that if it needs a certain amount of energy to start a train, it needs about nine-tenths as much to make an emergency stop. With local or accomodation trains the expense would be enormous therefore, to so frequently reduce the speed as soon as it reached 120 miles an hour, if the electric current were used as the braking power. In the experiments I refer to, the momentum of the train was used for braking, while the electric current was merely the agent for controlling the application of the force of momentum. This force is very cheap and plentiful, and even with the crude apparatus employed, emergency stops were made in which the cost thereof was simply that of an electric current of two amperes acting for less than one second.

MR. CROSBY:—In order to have any kind of service, whether you try it automatically or not, you have got to have it safe enough for a man to be on there. That is, you must not have so many accidents as would make it unsafe for a man to go on there, or your business would be gone. If you have an engineer he can have a man to sit by him and another man by him, and you go on till finally you have a passenger train. [Laughter.] I do not consider it practicable to run a train that is of any size, enough to make it commercial, without a man on it. I think the enormous complexity of an automatic system that would take care of the entrance into a city and the departure from a city and of all the varying conditions of service, without a man on board, is really beyond anything that would stand up. You might put it all down to work and have trains on it, but I think the complexity would soon break it down. As to the desirability of fast trains for passengers I must disagree with the gentleman who last spoke. I see a gentleman from Chicago who shook his head very emphatically when the statement was made that people did not want to travel so very fast. I think the limitation, so far as steam railways are concerned, is the ability of the engine to draw enough cars to accommodate people who want to go at that speed. That is what the New York Central people claim in regard to their limited train to Chicago They cannot run any more cars and have their engines make the speed that is wanted. People at Buffalo complain that they cannot get the fast service which people going from here to Chicago get. They have wanted additional cars put on, and the railway company has answered, "we cannot put on additional cars without sacrificing what we are aiming at, which is high speed."

MR. CHARLES WIRT:—I want to go on record as against these fast trains unless we can ride on them. I hope to see things fixed so that I can attend these meetings and get back to my

work in Chicago the next day. I can only come in about once in six months now. I ride on trains a good deal, and like to hold my watch in my hand and count the mile posts. I learned one very discouraging thing in doing this, which is, that a train which may make a mile in sixty seconds will average no more than thirty miles an hour, and I would be glad to know whether the ratio of maximum to average speed will be any different for the fast running that we are considering.

MR. F. L. POPE:—I think Mr. Thompson is certainly in error in stating that fast trains have been discontinued by our railroads to any great extent. As a matter of fact, I believe that there has never been a time in the history of the world when there were so many fast trains running in this country, or so many running in England, as there are to-day. I live on the route of the "Bound Brook line," which is certainly, all things considered, doing the best running in this country to-day—perhaps the best that is done anywhere. It is now operating a great number of fast trains, and they are increasing in number every year. Some very splendid running is done on that line. Coming over from Philadelphia only a few days ago, I timed three miles by the posts, the other side of Bound Brook, in two minutes and twenty seconds, which I think you will admit is very good speed for a train of five cars.

DR. L. H. LAUDY:—We have with us this evening a prominent railroad engineer, Dr. Dudley. If the Chair will call on him I think he will respond.

THE PRESIDENT:—Will Dr. Dudley favor us?

DR. P. H. DUDLEY:—I suppose Dr. Laudy prefers to have me tell you about some of the experiments I have made on the resistance of different trains. Those were made several years ago and not on as good tracks as we have at the present time. The resistance of the passenger train is given by the formulæ in books as seventeen or eighteen pounds per ton. I found on the Lake Shore Michigan Southern Railway and the New York Central & Hudson River Railroad that with trains of about 250 tons the resistance was only from ten to twelve pounds per ton at speeds of from fifty to sixty miles per hour. The resistance per ton is not nearly so great on those long trains, as on the short ones. With trains of two and three cars, sometimes it ran as high as thirty-five or forty pounds per ton, but with long trains it ran down to about ten and twelve pounds per ton.

The greatest variable that we find from day to day over the same track and same train is the wind resistance. I found in one special experiment I made in 1878 to see why the New York Central was not able to make its time with its trains, that on a still day, with the schedules they had, they were able to make time, but a wind of ten or twelve miles would retard the trains so that in running from Buffalo to New York they would lose two hours. The trouble at that time was that the boilers

were not large enough to quickly generate the steam required to meet the increased resistance. The trains would run up to a speed just about the capacity of the boiler and that would limit the speed they could maintain. Therefore when a head or side wind was blowing against them, the speed of the train would be reduced very materially.

One gentleman spoke about the fast speed of the local passenger trains. The fastest speed usually found on trains is in the local passenger service, for five to twenty seconds—especially those stopping every two or three miles. They often attain a speed for a few seconds as high as fifty-five or sixty miles per hour. Then they shut off steam and apply the brakes, to stop the train. The heavy trains of eight to ten cars, running long distances, rarely attain sixty miles an hour, that is on the ordinary schedules.

The greatest improvement that has been made in decreasing train resistance is in the improvement of the track by bringing up the standard of excellence and adoption of heavier and stiffer rails. I have been making a number of heavy rail sections for different railroads, and instead of making them simply heavy, I have made them very stiff which has reduced the deflection or wave motion under each of the wheels Comparing the resistance of the Chicago Limited Express on stiff eighty pound rails, with that of sixty-five pound rails which have been on the track some time, it makes a difference of seventy-five to one hundred horse power per mile. I have just made some ninety-five pound rails for a road, which will be very much stiffer than the eighty pound rails. We find now, that the condition in which they are keeping the line is so perfect that there is scarcely any oscillation on the best roads, and there is very little difference in the oscillation now in riding on a tangent or a curve. I should prefer a tangent for smooth riding, although the wheels press against the rails on curves, unless they are properly elevated, there will be some oscillation, though our present trains move very steadily on the best tracks. I have made careful experiments and with a track in good condition the heavy cars ride very steadily, leaving little to be desired.

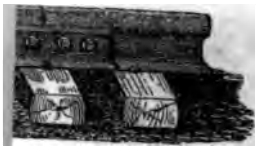
Mr. Crosby:—I should like to say now, knowing that Dr. Dudley is here, that what I learned from him very kindly given in conversation when I visited his very interesting dynagraph car, was more than I learned in any other way, and I think when Dr. Dudley shall have the time to publish the data that he has on hand, we shall know more about track construction and track resistance than is known anywhere else in this country, and I fancy in the world to-day. Of course I do not know all the data, but he has an enormous amount, and I am looking forward with very great interest to the time when he will give it to the world in published form, and it is very gratifying to me to find the facts that he has just stated, of the very great difference in power

required due to difference in tracks, and I was not very much more than quoting him when I said that half the battle was in the track construction, so far as the power required was concerned.

MR. C. O. MAILLOUX:—I can endorse the remarks just made by Mr. Crosby. I myself had the pleasure and the honor of visiting Dr. Dudley's dynagraph car some years ago. I likewise felt that Dr. Dudley had it in his power to confer upon the railroad fraternity countless boons and favors. I said to some friends that if we could only get Dr. Dudley to run his car on a street railway track we could learn from it revelations which I believe words would be inadequate to express. [Laughter.]

I think that there are a great many things which are so good in Mr. Crosby's paper that it would be very difficult to select any of them for comment and reference. As for criticism I do not believe we could well undertake it unless we had been with him through the long tedious course of experimenting, and the still more tedious course of calculation, designing, etc. Anybody who has been in the business—and I see there are several such here—knows what a task and a series of problems the development of electric traction is. But while I cannot dwell upon all the good things, I think that there is one particularly good thing in Mr. Crosby's paper, which is the statement that the track is half the battle. Mr. Crosby is, I think, very modest. If I had made the statement myself, I would have said the track is nearly the whole of the battle. I remember hearing our Past-President, Mr. Franklin L. Pope, whom we have the honor to have with us this evening, telling me in a very sententious way that the street railway people seem to feel perfectly satisfied not only to have grades of seven or eight per cent., etc., but to have one at the end of each rail. [Laughter.] My experience has been that on many roads they are not even satisfied with that, but they have one between every pair of ties or several in each rail. Now in the matter of designing electrical street cars particularly, we have to contend with all those difficulties and we have to resort to theoretical reasoning, oftentimes, for want of practical data. That is the reason why I prayed within my inner consciousness that some man like Dr. Dudley might come before us and give us facts and data on which to base calculations. I found at the outset, as my colleagues doubtless did, that there was absolutely nothing to guide one or to base any calculations upon. I was therefore compelled, as doubtless they were, to make some crude preliminary experiments out of which I might have data, which, though far from conclusive or satisfactory, were yet far better than none. Now I found this—which I state simply in the hope to draw out the experience of others—that the ordinary data in relation to the traction on street railroads are entirely out of the way, entirely unreliable. Various estimates have been given by civil engineers and others for the electrical traction per ton for a street car. The most authentic figures, if my memory serves me

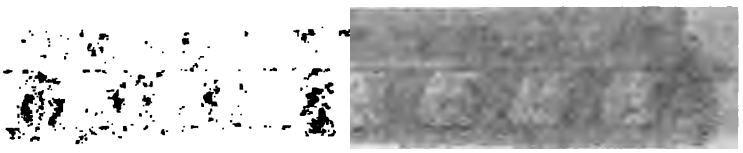
right, placed the traction at something like 13 pounds per ton. In practice, as the result of the crude traction-dynamometer experiments just mentioned, I have found the traction below twenty pounds with a [illegible] considerable weight. I have found [illegible] over thirty pounds

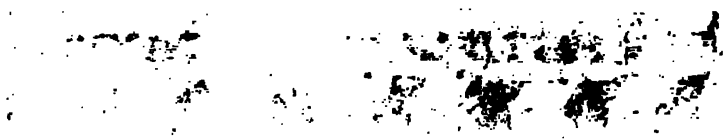


and in the interest of the West [illegible] Boston. It was very soon found from the actual power expended that the traction factor did not agree with the figures given in the text-books. In order, if possible to determine the value of this factor under ordinary conditions of level track, we secured a pair of powerful horses from the street railway company, that

required due to difference in tracks, and I was not very much more than quoting him when I said that half the battle was in the track construction, so far as the power required was concerned.

MR. C. O. MAILLOUX:—I [illegible] endorse the remarks just made





than none. Now I found this—which I state simply in the hope to draw out the experience of others—that the ordinary data in relation to the traction on street railroads are entirely out of the way, entirely unreliable. Various estimates have been given by civil engineers and others for the electrical traction per ton for a street car. The most authentic figures, if my memory serves me

right, placed the traction at something like 13 pounds per ton. In practice, as the result of the crude traction-dynamometer experiments just mentioned, I have found the traction below twenty pounds with a car having a considerable weight. I have found that the traction per ton reached in some cases over thirty pounds and it has gone as high as forty pounds. Calculating the traction theoretically, using the coefficient of friction on the axle, the weight, and the well-known formula, (since the power exerted is expended practically in overcoming the friction of the axles), one would expect to find a much lower traction. As a matter of fact, calculation gives a figure which is nearly in accordance with the data given by the engineers; yet practice always gives a higher figure by fifty per cent. and often more. I could not account for it unless it be that owing to the poor condition of the tramway rail surface, as the result of dirt, dust, depressions, etc, we have a greater amount, and a greater amplitude of rise and fall of the car as it rides over the minute excrescences on the track. I concluded from this that as the car is being continually and repeatedly lifted slightly, we must introduce into the formula for traction a factor which is a function of the weight of the car itself; in other words, that the traction per ton increases with the weight of the car. I mean that the traction per ton for a car weighing three tons might be twenty to twenty-five pounds, while that of a car weighing seven or eight tons under the same circumstances might be probably thirty or thirty-five pounds. This view might explain the lower figures found for the traction of horse cars, these being of so much smaller weight

I would like to ask Mr. Crosby in regard to the initial torque; that is the amount of traction which is necessary to overcome static friction; in other words what he finds the difference between static and kinetic friction to be. I have found in my experience that the initial traction went up as high as 80 to 100 pounds per ton. I would be very much interested in having the results of Mr. Crosby on these matters, because I think it is a subject on which we are all groping for much needed light. We are still working in comparative obscurity. Consequently any slight illumination will be very acceptable, not only to myself, but to all interested in the development of an art which is only in its infancy.

Mr. Geo. B. Prescott, Jr.:—I can confirm to a certain extent what Mr. Mailloux has said about the unreliability of the traction figures for street cars that are given in some text-books. Several years ago I had occasion to try a number of experiments with a storage battery car for the Sprague company and in the interest of the West End Railway Company of Boston. It was very soon found from the actual power expended that the traction factor did not agree with the figures given in the text-books. In order, if possible to determine the value of this factor under ordinary conditions of level track, we secured a pair of powerful horses from the street railway company, that

were specially trained to start very gently and pull steadily. We took the car out on a nearly level track, and with an ordinary spring-balance attached between the car and the horses, endeavored by leaning over the dash-board to read the indications of the balance. [Laughter]. This was found to be practically impossible owing to the rapid fluctuations of the indicator. Then followed a very amusing incident. Several gentlemen had been invited to ride in the car and witness the experiments. At this stage a rope was procured and the guests invited to act as motive power, much to the amusement of the street urchins who unmercifully guyed the gentlemen in silk hats and top-coats who were acting as car horses. A number of interesting readings were secured which seemed to indicate that the condition of the track was not at all uniform, and it seemed to happen that when we were going to start the car it would always be blocked by a spike. As this meant raising the whole weight of the car in starting, it was frequently impossible for the men who were pulling the car to get it over the spike, and we would have to push it back and get up momentum; which probably goes to show that the traction coefficient is a function of speed. [Laughter.] These experiments were continued the whole afternoon, and on a perfect level we found the traction to be somewhere in the neighborhood of 30 pounds to the ton for a car weighing about six tons.

Dr Wm. E. Geyer:—I must ask a question which will subject me to the criticism of being an electrical heretic. It has been stated over and over again this evening that in the case of electric locomotion, the main battle is the track. Now if the same attention were paid to the track, I would like to ask Dr. Dudley or any other railroad man, what in their opinion would be the possibilities with ordinary steam locomotion, if we are willing to spend large sums of money on the track and on maintenance, approximately as proposed by Mr. Crosby.

Dr. P. H. Dudley:—I do not know how to answer the gentleman. Our experiments are not sufficiently carried out to determine yet all those points. About the best results are those that I have given you, that is, a reduction of nearly one hundred H. P. per mile, compared with the ordinary sixty-five pound rail. I have designed some one hundred and five pound rails which are nearly one hundred per cent. stiffer than the eighty pound rails. We will probably save on the fast express trains nearly two hundred H. P. per mile, as compared with worn 60 or 65 lb. rails. We have not rolled such a large rail yet; but it will not be long probably, before that will be done. All trials show a very material reduction in train resistance with the heavy rails, as we increase the stiffness. I think it will be perfectly safe on a track of the 105 lb. rails, to run 120 miles an hour. I have ridden several times seventy-five miles an hour on heavy rails and did not feel it nearly so much as on the light rails when

running forty-five miles an hour. There is scarcely any oscillation. The track is in perfect line and the joints are all kept up. I might show you on the black-board the shape of some of the rails we find in tracks. Taking a thirty foot rail on our best tracks; its surface to the eye would be represented by a straight line. [See Fig. 1.] The joint is well maintained in uniform surface with other portions of the rail Take even sixty-five pound rails, after they have been run over a little while, if the joints are laid opposite, we find the rails in this form. [See Fig. 2.] Down at the joints and high in the center. Now, of course, as the locomotive or train is passing over this depression, it further deflects and the wheels strike on the receiving end of the rail and cut out a piece from the head of the rail. This has been very common. Some times the effect is sufficient also to bend the rail down at this point. The apparatus I have on my car ejects paint on the rails where there is a certain deflection. In Fig 2, it will be noticed the receiving ends of the rails are commencing to cut out. By ejecting the paint, the trackmen have been able to surface the track so that on rails 15 or 16 years old, they have not only checked the cutting out, but have brought up the surface of the rails to a much better condition, and they are nearly in a straight line at the present time One serious trouble with many of the railroad people is, that, while they want a heavy rail, they never consider the question of stiffness. I distribute my metal so as to make the section very stiff, not allowing so much in depth of the head of the rail as usually is done, but widening the head, because when these rails have been in the track a few years, they become worn very unevenly, and must be removed from the track. I am making the heads on that account very much broader and the sections stiff, so the deflection of my 75 and 80 lb. rail is less than one-tenth of an inch in the track. We mark all those rails for any deflection exceeding three thirty-seconds of an inch, under a load of six tons per wheel. That is about as close as the trackmen can work on a sixty-five pound rail. On an eighty pound rail they can work closer, but require great skill. The ejection of paint also shows where there is a rotten or loose tie. The form of rail where the joints are low, and high in the centre I call the first form. Then there is another form in which the joints and centre are low, and high at the quarters which I call the second form. [See Fig. 3.] Then there is another rail of which the surface is all full of waves. I call that the third form. [See Fig 4.] A few years ago that was a very serious matter. We are often able to tell from the diagrams in the car, what mill made the rails as we run over them. That feature has been materially improved by the mills. You will see at once, with trains running over rails of these various forms and with the deflections, there is a large amount of force lost. Rails high in the centre to the eye, will be loose on the ties, so that there is quite a wave under each wheel. Even with very good

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track we find that under each wheel there is a depression depending on the weight on the wheel and speed of train.

MR. LOCKWOOD:—Before the discussion is concluded, I would like to call attention to just one point more which seems to have escaped attention so far, though I have no doubt that those of us who are experts in electric railroading have not overlooked it, and that is that with the increased use of electric locomotives, we may expect to have better preserved tracks, and tracks which, moreover will hold out much longer; because in the transfer of the rectilinear motion of the steam-piston to its connecting rod and in its transformation to rotary motion in the wheels, there is a heavy pounding effect, which will certainly be absent from the motor having a rotary motion at the outset.

MR. FRANK J. SPRAGUE:—I was not here early enough to hear Mr. Crosby's valuable paper, but I have gathered the gist of some of the points from the criticisms made. I wish to take the decided position that no automatic system of high-speed service for either passengers, freight or mail, will ever come to a commercial issue. I will go further still: the moment you deny to a man the privilege of traveling 100 or 150 miles an hour you will have a flood of applications from people that want the opportunity. I know very well what Mr. Crosby's feelings are, and perhaps it was his belief in high-speed service which brought him into the railway field, and I will relate an incident which will perhaps be new to you. I do not remember what year it was that Mr. Crosby left the army, but it was six years ago that I was married, and he was my groomsman. I saw him but one day, and a year or two later, I happened to be in New York City at the time when the Richmond road was under consideration, and I was casting about for some one to associate with me in my work. It was just after some experiments on the elevated railroad had been carried through. Mr. Crosby flashed across my mind. He was then down in New Orleans. I said to my wife: "The time has come when I must have some one to help carry the load, and I want a man about my own age, and one who has nerve, and grit, and coolness so that if he gets on the front of a car that is going 75 miles an hour, and there is danger ahead, he will stay there." I said "do you think Mr. Crosby is that kind of a man? Has he that kind of nerve?" She replied, "I think he has" I went to the office, and telegraphed to him: "Can you come on to New York at once for a week?" He telegraphed. "Yes, I will come." He came on. We had a conversation, and the result was that Mr Crosby resigned from the engineer's corps, and stands to-day as one of the foremost exponents of high-speed service.

Mr. Crosby spoke of the possibility of returning the energy of the train to the line, and that it would have very little importance in dealing with the particular class of service of which he has spoken—high-speed service over long distances. That is very

true. I think perhaps the value there of a shunt machine if used, would be on account of the constancy of the speed. That is, it would always be constant within a certain very small limit, especially with machines of the exceedingly high efficiency which would be used under those circumstances. That is where I think, perhaps, the shunt machines would be used. But on local rapid transit service, or service like that of the elevated railroads of New York City—which is not very rapid—but where we have stops every third of a mile, then the importance of returning energy to the line becomes at once very great, because from careful calculations I know it to be possible to return about forty per cent. of the total energy used, back to the line, and the facility with which this can be done increases very rapidly with the size of machine which is used Where you can use shunt machines, and excite your field-magnets up to a very high saturation, and have a very reasonable latitude of variation, the criticism that was made that there would be a loss in energizing the field-magnets more than when in using series machines, I do not think holds particularly good, for this reason: that the total energy used in the field magnets forms a very small proportion of the total amount of energy used on the line, and if the returned energy is forty per cent. of the total, and the field energy of the magnet is only two or three per cent. of the total, the value of this method seems apparent.

The question of traction which has been so ably spoken of by Dr. Dudley has two entirely different bearings If you are dealing with street car service, it is one thing. If you are dealing with high-speed, clean rail service, it is an entirely different thing. I have found, so far as my practical experience goes, that the dirt on the track, the snow and mud, and the grades, were the questions that determined the power of the motors. When you consider that with twenty pounds traction per ton, the work on a one per cent. grade is equal to the work of traction, and that our street car service demands 12, 13, and even 14 per cent. grade work, the question of rail traction on a level amounts to very little in street car motor design, but it does amount to the greatest possible importance in high-speed service.

Mr. Crosby was speaking, just as I came in, of the influence of currents on traction where the ground circuit is used. I was once a very zealous upholder, especially through our commercial agents, of the importance of using the ground circuit, because of the value of adhesion. My belief was founded on the experience I had with a very dirty track, for which the electricity was perhaps a sort of broom to brush away, or a fire to consume the dirt. I am now of the opinion that the passage of the current or at least so much current as is necessary for actual traction, has little actual influence on actual adhesion with clean rails. If it has any influence, it simply acts as a cleaner of the track—nothing more than that. [Applause].

MR. J. STANFORD BROWN:—The question of grades has come up and been discussed, but the question of curvature has not, unless it was taken up before I came in. I would like to inquire what limitations would be put upon that in this very high-speed service.

MR. CROSBY:—In the paper as read, there is a calculation for the limit of curvature as a question of safety. Now there is a very important question as to the effect of curvature in increasing the friction coefficient. On that I have been able to do nothing more than take the text-book formulæ, and I am not at all sure that they are right. The matter of safety I have discussed in the paper. The power I have assumed ordinary coefficients for. As to the values of the coefficients in street railway work on straight track; I found with a dynamometer on an ordinary flat rail, about twenty-five pounds average per ton for a car weighing about twelve thousand pounds, that is, a street car, on an average street car track. As to the starting effect, I started a car equipped with the ordinary Sprague motor with fourteen amperes by having a water resistance external to the car. Of course all cars as they are made by all the companies that I know of, take a larger current than that to begin with under any conditions, simply because C is equal to E divided by R. You have so much R and so much E and you will have so much C. But that current is many times larger than is necessary to start a car. Those 14 amperes started a car on what was practically a level, rusty track, then I put a dynamometer in to check it, and as I remember now got about 175 pounds as the effort, applied gradually through a rope and a dynamometer to the starting of this car. That would be about thirty-five pounds—from thirty-five to fifty pounds per ton, and it checked exactly with the calculated torque resulting from the 14 amperes which were used electrically. In other words I started once mechanically by men hauling on the dynamometer, and then electrically, and they checked very well running somewhere to 175 to 200 pounds for five tons.

Now another gentleman has not I think been answered as to the question he raised respecting the feasibility of steam as compared with electric traction in case every possible improvement is made. I assumed in the paper I prepared previously on the subject, that the traction coefficient would be the same for the same pull no matter how exerted. It is so much per ton that has to be exerted. Now the way I took hold was to calculate this very thoroughly—this cumulative load with steam, the coal necessary to be carried, and the water and the surface opposed to the atmosphere in increasing your speed, and I formed a table which I believe is reliable showing that at varying ratios for efficiency for electric locomotive machinery, and assuming what we know to be the average efficiency of the steam mechanism at such and such speeds, that you will have an economy greater in the one case or the other, and it runs this way;

that if you can get ninety per cent. efficiency out of your electric service and have a frequent service—that is, of course an indefinite term, but I define in my calculation a particular frequency which is not excessive as compared with the local traffic, say, of the New York Central road—with such a frequency with ninety per cent. efficiency and at twenty miles an hour, the electrical propulsion is slightly more economical than steam propulsion. Now as you increase your speed the relative value of the electrical propulsion increases enormously, and finally at high speed, say 120 miles, your electrical propulsion is something like six times more economical than steam propulsion. considering steam propulsion even possible But it is not possible to propel ordinary trains by steam at 120 miles for any length of line worthy of consideration, because the amount of coal and water that you are required to carry has reached a quantity that would require two or three tenders to take care of. You can see readily that it is a cumulative thing. Every pound of coal you put in requires some more coal to be burned to pull that pound and you have to carry it, and also water, so that when you get to the high speeds it is out of the question and we have nothing to look for at very high speeds, save electrical propulsion But if we come to comparatively low speed service and a comparatively infrequent service the steam will beat us. [Applause].

MR. PEIRCE:—Do I understand Mr. Crosby then to mean that for heavy freight service steam would be more economical than electricity.

MR. CROSBY:—Yes; I think for heavy freight service, unless its frequency is far beyond anything we have to-day, it can be carried on more economically by steam than by electricity. The table I prepared, I calculated with a great deal of care for the cost of generating so much power in the one method and in the other, and I also introduced as nearly as I could the dead weights needed for producing that power by the two agents. Those I will say are factors that have to go into it. The cost of producing one horse power hour and the number of tons of paying freight that one horse power hour can haul when that horse power hour is expended by the one means and the other. Those two things being considered, put them together in the equation and you have at once the speeds at which the one or the other agent becomes more economical.

MR. SPRAGUE:—I heartily agree with Mr. Crosby in his opinion regarding the freight service—that it will for a long time be more economically carried on by steam, than by electricity; likewise long trunk transportation You must understand that electric transmission is going to bring up a new system of train operation, and train despatching.

I think that in justice to Mr. Crosby I should explain why he was said to "have nerve" It was related that at a balloon ascension in the city of New Orleans, Mr. Crosby got away in

the balloon without the operator, because he was bound to go. I feel, in discussing this subject very much as that operator probably did. Mr. Crosby on this question of rapid transit has run away from me, simply because I know of it only through the discussion that is going on.

MR. THOMPSON:—If I understand Dr. Geyer's question it is: "All other things being equal, why can we not safely use as high speed with the locomotive as with the electric motor?" There seems to be one certainty in favor of the electric motor as regards the matter of safety not found in the case of the locomotive. High speed means more danger of the numerous parts such as connecting rods of the locomotive breaking; it means more danger of boilers exploding, etc. Where the armature is mounted on the car axle as proposed by the speaker, the electric motor has no extra moving parts over those found in an ordinary car—as against, I don't know how many moving parts in the locomotive,—but a great number intricately involved; so that an electric train would have a very high per cent. of safety over the steam locomotive, whether the present track is improved or not.

THE PRESIDENT:—This discussion is an exceedingly interesting one, but we have some other matters to come before us this evening, and it is past ten o'clock already. Mr. Franklin Leonard Pope, I believe, has agreed to tell us something about the history of Mr. Davenport's little locomotive here, which perhaps will run a hundred and twenty miles an hour. I don't know whether he thought of doing it or not.

[Mr. Pope spoke as follows:]

THE INVENTIONS OF THOMAS DAVENPORT.

BY FRANKLIN L. POPE.

The year 1837 marked a very important era in the history of the industrial development of electricity. During that year, two of the most extraordinary inventions of the present century made their advent in this city—the electric telegraph of Prof. Samuel F. B. Morse, and the electric motor of Thomas Davenport. The motor came first. Davenport, a self-taught Vermont blacksmith, who had invented and constructed his machine in a remote country village in a crude form as early as 1834, came to New York in February, 1837, bringing with him some of the machinery which had been made for the purpose of exhibition, by himself and Ransom Cook, with a view to enlisting capital to build a large motor. During the season of 1837 he occupied rooms in the city where he exhibited models of his motor and various other electrical inventions. Articles were published in the newspapers of the day, and his exhibitions were attended by a great number of people. In letters written about that time, Davenport mentioned that one of the most frequent visitors was Professor Morse.

With the history of Professor Morse and his inventions of the telegraph the world is quite familiar. With the history of Davenport and of the invention of his electric motor—as I soon discovered in endeavoring to obtain information about it myself—scarcely anything was known. This difficulty ultimately led me to interest myself in trying to find out more about the matter. In this undertaking I was fortunately very much more successful than one can reasonably expect to be, in attempting to resuscitate the history of an invention which has lain unknown for the best half of a century.

The first newspaper notice of the electric telegraph which

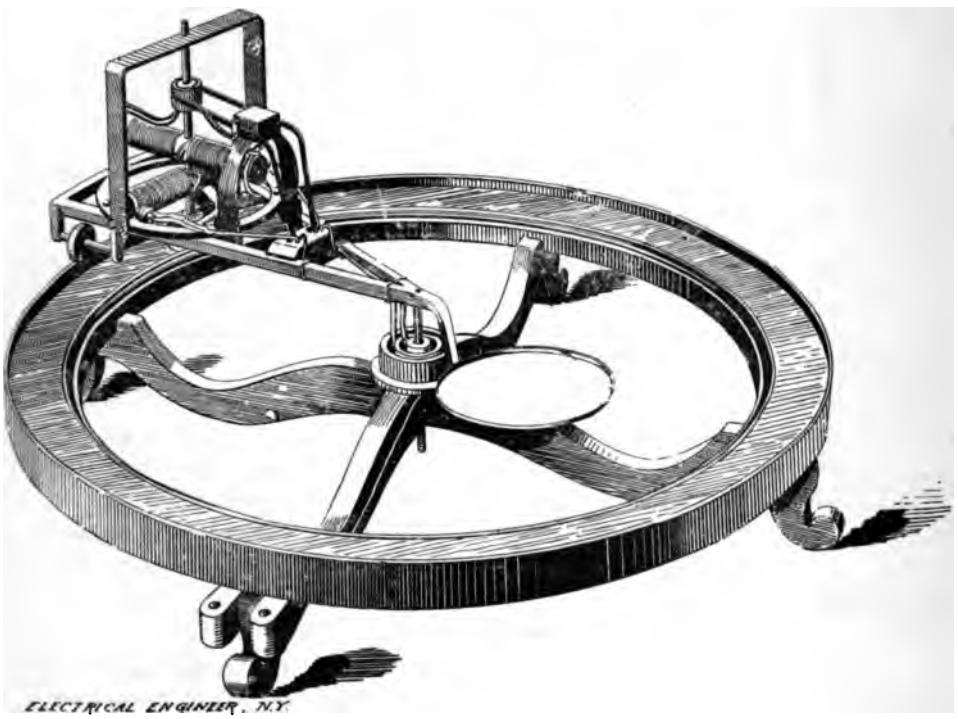
appeared in America was in the *New York Observer* in the summer of 1837. I will read you an extract from it:

"A gentleman of our acquaintance several years since suggested that any intelligence might be communicated almost instantaneously hundreds, if not thousands of miles, by means of very fine wires, properly coated to protect them from moisture and extending between places thus widely separated. It is well known that the electric fluid occupies no perceptible time in passing many miles on a wire, and if it is possible by connecting one end of the wire with an electrical or galvanic battery to produce *any sensible effect whatever* at the other, it is obvious that if there are twenty-four wires, each representing a letter of the alphabet, they may be connected with the battery successively in any order, and if so connected in the order of the letters of any word or sentence, that word or sentence could be read or written by a person standing at the other end of the wires. All the paragraphs of a newspaper could thus be touched by a man in Philadelphia, and the contents, *verbatim et literatum* conveyed to New York as fast as a compositor could set up the type!"

It has always been a mystery to me what invention was referred to in that article. *The New York Observer* was edited at that time by a brother of Professor Morse. The "gentleman of our acquaintance" referred to, was no doubt Professor Morse, but what was meant by the telegraph of twenty-four wires I could never make out, because we all know that Morse never entertained any idea other than of a single-circuit telegraph at any time from the beginning of his experiments in 1835, or earlier. But I received a few days since a letter from a gentleman who had read some articles recently published by me in reference to Davenport's work, in which he stated that he visited Davenport's exhibition in the summer of 1837, here in the city of New York, and that he himself saw in that exhibition a model of a telegraph of twenty-four wires invented by Davenport, and that model must undoubtedly be the telegraph referred to in the extract I have just read.

In pursuing my investigations into the history of Davenport's work, I was fortunate enough to find that several of his original models were still in existence. One of these models, a circular railway two feet and a half in diameter with a locomotive travelling on it, similar to the one which we have here tonight but very much more finely finished, I was able to satisfy myself by contemporaneous evidence was built in 1837. The one which is before you was found in the cabinet of the Female Seminary in Troy, formerly Miss Willard's. The records of the

institution show that it was purchased in 1840, but it evidently must have been built by Davenport himself prior to the more finely finished model of which I have just spoken, which was constructed in 1836–7 with the assistance of Mr. Ransom Cook of Saratoga, a very ingenious person and a finished mechanic. I think there is no doubt that the model before us was built in the early part of 1837 and possibly as early as 1836. Fortunately it is, as you see, in a very good state of preservation. I have had it mounted to-night with the same battery that was



THOMAS DAVENPORT'S ELECTRIC RAILWAY MODEL, 1837.

found with it—a three-cell Grove battery of pint cups, and I think we shall find that it is in tolerably fair running order. [The model was set in motion.] It does not go quite a hundred and twenty miles an hour, but it goes. This locomotive, you will observe, has a fixed field magnet below, and a revolving armature above, which is reversed twice in every revolution. So far as I am able to discover, that is a combination found in every practical motor to-day, which Davenport was the first to make known and to use in 1834. The motor is connected to the driv-

ing axle by bevel-gear. The field and armature in the model before you are connected in series In the other model of 1837 of which I spoke, they are connected *in shunt.* Among other models of Davenport was one having a horse-shoe field and a four-pole armature with curved pole-pieces, which is really quite an advanced type of motor. That also was built in 1837. Altogether, Davenport, between 1834 and 1840, built over 100 motors of different patterns and of varying sizes, scarcely any two of them alike. I discovered also, quite to my surprise, that he was undoubtedly the first to make use of the solenoid with a movable core as a means of moving machinery of any kind. He built in 1839 quite a large motor operating upon that principle, which from the best description I have been able to find of it, must have approached one horse power. He set that up at 42 Stanton St., in New York; attached a printing press to it, and started to publish the pioneer electrical journal of America—a small weekly paper called "The Electro-Magnet and Mechanics' Intelligencer," printed by electrical power. Through the kindness of Mr. Davenport's son, Rev. Willard G. Davenport, I have the pleasure, on his behalf, of presenting to the American Institute of Electrical Engineers two copies of that paper. [Applause.] These, so far as is known, are the only editions ever published; they are number one and number two. This was in 1840. I have received within a day or two an interesting letter from Mr. H. S. Davenport, a nephew of Thomas Davenport, still living in Vermont at an advanced age, who, as a boy, was with him a great deal and in that way became quite familiar with his work. I will take the liberty of reading extracts which give some idea of the manner of man Mr. Davenport was. He says:—

"Many of his models never left his shop and were but little known even at the time of their construction. They were only made to show to how many uses the power could be applied, and also to work from on a larger scale, if he could get pecuniary aid to do so. The different models which interested me most, at the several times I was in his shop, were a trip-hammer, a turning lathe and a machine for doubling, twisting and reeling cotton or silk, all at the same time. A circular frame fitted with two intersecting tracks, on which four miniature cork images glided around, he called his "puppet-show." He was naturally of a retiring disposition, but when waked up was very strong in argument. His two favorite subjects were nature and electro-magnetism. He considered magnetism the most important element in the creation of the universe and thought it would be, in its de-

struction. Magnetism kept the heavenly bodies in their places, and if that failed everything would be turned to chaos. He could see in every rock of the earth the battery of which it was composed. So also in the animal kingdom, the bones, muscles, and blood constituted a complete battery, which exercised a repulsive or attractive force with respect to another organism of the same kind. He was a great lover of fun and exceedingly fond of a joke. On one occasion he received an order from a party in Chicago for half-a-dozen bottles of electricity He said he knew by the tenor of the letter that it was intended as a joke, and he accordingly replied that he bottled up his *wrath* for such would-be ignoramuses as he was, but had no electricity for him."

I might go on and relate many other facts of interest which I have learned about Mr. Davenport and his inventions, but the hour is already late, and I will forbear. But I may remark in conclusion, that I find him to have been one of the most interesting characters with whom I have ever become acquainted, and I cannot but feel highly gratified that my attention chanced to be directed to the subject, in season to rescue at least some of the more important of his work from impending oblivion. In two or three years more it would have been too late. There are people now living, although at an advanced age, who knew him and remember him perfectly, but in a year or two more perchance they will be gone. [Applause.]

Mr. Ralph W. Pope:—Mr. President, I move that the gift of Mr. Davenport be accepted by the Institute, and that we tender our sincere thanks to the donor for these very interesting relics of the early days of electrical investigation.

[The motion was carried.]

The President:—The report of the Standard Wiring Committee and the report of the Committee on Amendment to the Patent Law are to come up. It is now half-past ten.

Mr. Lockwood:—Mr. President, I move we adjourn.

[The motion to adjourn was carried.]

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, March 17, 1891.

The 55th meeting of the American Institute of Electrical Engineers was held this date, at No. 12 West Thirty-first street. The meeting was called to order by President W. A. Anthony.

THE PRESIDENT:—Before proceeding with the regular business of the evening, the Secretary has an announcement to make.

THE SECRETARY [Mr. R. W. Pope]:—The sad duty falls upon me to announce officially, what is already known to nearly all of you, the death of our esteemed young member, Mr. Thomas P. Conant of this city, District Engineer of the Edison General Electric Company, who died on the 24th of February, the date of our last meeting. At the meeting of Council this afternoon the Secretary was instructed to prepare a suitable sketch of his life for publication in the Transactions, which will be read at the next meeting of the Institute.

[Mr. J. J. Carty then read the following paper on Inductive Disturbances in Telephone Circuits.]

A paper read at the 55th Meeting of the American Institute of Electrical Engineers, New York, March 17th, 1891. President W. A. Anthony in the Chair.

INDUCTIVE DISTURBANCES IN TELEPHONE CIRCUITS.

BY J. J. CARTY.

It was not known until 1838 that the earth might be used instead of a return wire in the circuit of a telegraph line. This fact was discovered by Steinheil, while making experiments to determine whether the track of a railroad could be used to complete the circuit instead of a return wire. Steinheil's discovery has been considered as one of the most important in the art of telegraphy, and since his time it has been the almost universal practice to use the earth as a return in telegraph circuits. When, however, the earth return is used with the telephone, it is found that owing to the extreme sensitiveness of that instrument, many foreign currents, which would produce but little effect on telegraph instruments, become a source of serious trouble. For this reason, in telephony it is often necessary to use a complete metallic circuit. Thus it has come to pass that a return to the original practice of employing metallic circuits, as in the case of the crudest telegraph instruments, is now regarded as a most important improvement in connection with that most highly developed of telegraph instruments, the telephone.

To obtain complete freedom from inductive disturbances in the telephone, it is necessary not only to use a metallic circuit, but to place the two wires composing the circuit in a special relation to the source of disturbance. One of the methods of so arranging the wires is shown in Fig. 1.

In this case the two sides of the circuit L^2 and L^3 are twisted spirally about each other, so that their average distance from the disturbing wire L^1 shall be the same. With such a plan, the tele-

phones *a* and *b* are not affected by induction from the wire L^1. This fact has usually been explained by assuming that a current, commencing to flow in L^1, tends to induce two currents in the op-

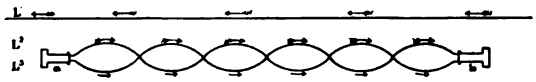


FIG. 1.

posite direction to itself, one in the wire L^2 and the other in the wire L^3; that both currents are of the same strength because the average distance of L^2 and L^3 from the disturbing wire L^1 is the same; and that the telephones *a* and *b* are silent because they are acted upon by two equal and opposite forces. The above theory seems at first sight to be correct, and indicates the line of explanation adopted in many works treating of the subject.[1]

I have made a number of experiments which seem to prove that in the case described, the inductive action, instead of tending to produce a simple current in each side of the metallic circuit, actually does produce a number of different currents in each wire and in both directions.

In Fig. 2, L^2 and L^3 are two well insulated copper wires, 500 feet long and three feet apart. These wires are joined together at each end through an ordinary telephone. L^1 is a well insulated wire similar to the other two and placed within one-half an inch of L^2. At one end of L^1 is placed a Blake transmitter T and at the other end is placed a subscriber's bell S; L^1 being grounded at both ends as shown. When the transmitter T is operated, either by speaking into it or by vibrating a powerful tuning fork in front of it, disturbances are produced at the end telephones.

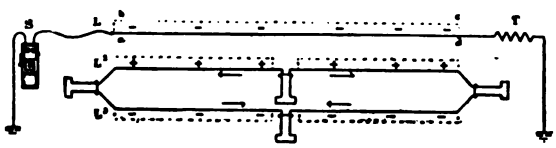


FIG. 2.

If telephones are inserted at the centre of L^2 and L^3 it is found that at those points no sound is heard, while the noise of the end telephones continues as before. This constitutes a simple case

1. "A Handbook of Practical Telegraphy," R. S. Culley, 1885, p. 330. "The Telephone," Preece & Maier, 1889, p. 134. "Die Technik des Fernsprechwesens," Dr. V. Wietlisbach, 1886, p. 135.

of inductive disturbance, and can only be explained by assuming that the wire L^1 acts electrostatically upon the circuit composed of L^2 and L^3, or in other words, that we are dealing with a system of condensers in series, of which L^1 constitutes one plate, the earth another and the wires L^2 and L^3, two intermediate plates which are joined together through the end telephones.

Let us assume that at a given instant, the height of potential along the wire L^1 is represented by the lines *a b* and *c d*, and the charge upon L^1 by the rectangle *a b c d*. The presence of this charge, which we will say is of the minus sign, is accompanied by an equal but plus charge on L^2. This produces a minus charge on the wire L^3, which in turn acts upon the earth. Now we will assume that the potential on L^1 is reduced to zero. This causes a restoration of the equilibrium in the circuit composed of L^2 and L^3, which is accompanied by a set of currents as represented by the arrows. The plus charge on L^2, flowing through the end telephones, neutralizes the minus charge on L^3, thus leaving a

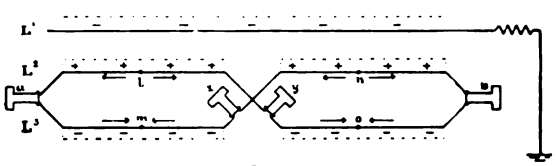


FIG. 3.

neutral point at the centre of each of the wires L^2 and L^3.

In this and the succeeding experiments the induced charges are represented by rectangles. This is not a strictly accurate method, but as an error in this respect only affects the result quantitatively, and cannot alter the conclusions, I have adopted it on account of the simplicity of treatment which it permits.

The potential along the wire L^1 at a given instant is represented as being constant, as it is found that owing to the high impedance in the instrument s, there is practically no fall of potential in the wire itself. Furthermore, it was found in the experiments about to be described that the results were the same whether the disturbing wire was open or connected to earth through the bell s. There would be a fall of potential along the wire L^1 if it were of such a length as often found in practice, and the distribution of the charge would be modified accordingly. But as this fact would only affect the location of the neutral points, the question of the fall of potential will not be taken into consideration, and the disturbing wire will be shown open at one end.

Fig. 3 shows the same circuits as used in the previous experiment, except that the wires L^2 and L^3 are caused to change places at their centres but are continued in the same plane, as in Fig. 2. This is what is called a "transposition" The telephones at the ends and in the middle are retained as before. Under these conditions, when the transmitter is operated, a diminished sound is heard at *a* and *b*; and *x* and *y*, instead of being silent as in the first case, now emit a sound of the same intensity as *a* and *b*. When telephones are placed at the quarters *l*, *m*, *n*, *o*, the sounds at *a b* and *x y* continue as before, but nothing is heard at *l*, *m*, *n*, *o*. The effect of this one transposition has been to reduce the disturbance at the end telephones *a* and *b*, and to cause a shifting of the neutral points from the centres to the quarters, and an increase of their number from two to four.

An examination of the induced charge shows how these results have been brought about. It will be seen by reference to Fig. 3, that there is on the first half of L^2 a positive charge, and on the second half a negative charge, and on the first half of L^3 a negative charge, and on the second half, a positive charge. When the positive charge on L^2 discharges, half of it goes through the telephone *a*, and half through the telephone *y*. The positive charge on L^3 escapes in a similiar manner, half going through telephone *x*, and half through telephone *b*. This produces four currents, two starting from point *l* and two starting from the point *n*. One of the currents from *l* meets a current at *o*, coming from the point *n*, and one of the currents starting at the point *n* meets a current at *m*, which started from *l*. The currents flowing through the end telephones in this case are not so strong as when no transposition is employed, because they are due to an induced charge which is represented by an area half as great. It is found that as the number of transpositions is increased the number of neutral points is also increased, and that the area representing the charge, which escapes through the end telephones is reduced. To obtain silence, therefore, it is necessary to increase the frequency of transpositions until the discharge through the end telephones is so small as not to produce sounds therein. Fig. 4 shows a circuit containing three transpositions, one at the centre and one half way between the centre and each end. In this case, if we could neglect the resistance of the telephones, the current going through the ends would be reduced to one fourth of its original proportions, and eight neutral points would be produced, one in each wire at the centre of each transposition.

In practice the impedance of the telephones must be taken into consideration, so that if in Fig. 3, the middle telephones x and y were omitted, the neutral points would be found to move towards

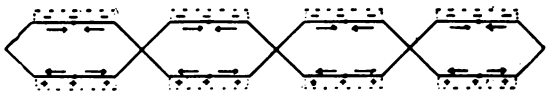


FIG. 4.

the ends, and the currents flowing through the end telephones would be correspondingly reduced.

According to this theory of transpositions, if the instruments and distances between wires on a given circuit remain constant for a given period of alternations in the disturbing wire, the number of transpositions necessary to obtain silence will depend on the E. M. F. of the disturbing wire, and the specific inductive capacity of the dielectric. An increase in the value of either of these factors will, if silence is to be maintained, require additional transpositions, their number depending upon the value of the change which is made.

Where the disturbing wire is placed at an equal distance from both sides of a metallic circuit, no noise is produced in telephones located in that circuit, and a balance once being obtained, it is independent of both the E. M. F. of the disturbing wire and the specific inductive capacity of the dielectric. Fig. 5 shows such an arrangement of circuits —L^2 and L^3 are the two wires composing the metallic circuit placed the same distance apart as before, and the disturbing wire is at an equal distance from both. When the disturbing wire is in operation, no sound is heard at the end telephones, or at telephones located at the centres. This may be accounted for by assuming that at a given instant a negative charge is on the disturbing wire, which produces a positive charge on the inside of L^2 and L^3 and a negative charge on the outside of

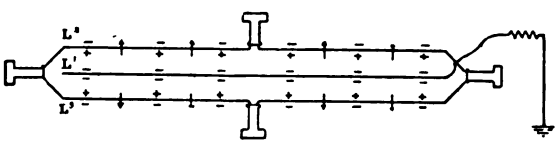


FIG. 5.

those wires; and that when the charge is removed from the disturbing wire, a set of currents is set up in the wires L^2 and L^3 in a direction at right angles to their axes, as shown by the arrows. In

this case the flow is lateral, and no current passes through the end telephones or through telephones located at the centres. It will thus be seen that this method of arranging wires differs essentially in its action from the plan of using transpositions. Unfortunately, however, its practical application is limited to two circuits.

Where the disturbing wire occupies the position shown in Fig. 5, the flow in the conductors is lateral only when the wires L^2 and L^3 are insulated from the earth. If a ground be attached to the centre of L^3, as shown in Fig. 6, the flow of current becomes longitudinal, and the telephones *a* and *b* are found to be affected by loud disturbances, while the telephone *x*, at the centre of L^2, is found to be silent. This is because the disturbing wire, which we will say is negatively charged, induces a positive charge upon L^2 and L^3 and a negative charge upon the earth. The discharge in this case is effected by two currents starting from *x*, which thus becomes a neutral point, and passing through the end

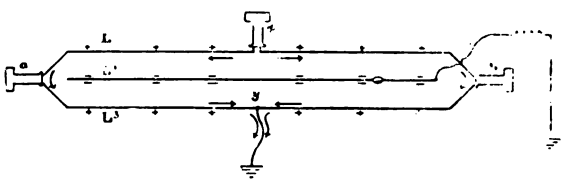


FIG. 6

telephones to the ground as shown by the arrows. If the ground be removed from the point *y* toward the telephone *a*, the neutral point will be found to move toward telephone *b*, and if the ground be put at the centre of resistance of the telephone *a*, the neutral point will be found to be at the centre of the telephone *b*. This is well illustrated in Fig. 7, where *a* and *b* are telephones of special construction, admitting of the attachment of grounding keys K^1 and K^2 at their respective centres. In this instance, when the disturbing wire is in operation and both keys open, no sound is heard at any of the telephones, the flow of current being lateral. If the key K^1 be closed, sound is immediately heard at telephones *x* and *y* located at the centres of L^2 and L^3, but the telephones *a* and *b* are still silent. This is because the charge and discharge take place along the conductors L^2 and L^3 to and from the earth at K^1, thus passing through *x* and *y*, *a* being silent because the currents go through it differentially, and *b* is silent because it is located at a neutral point. To prove that current flows through

telephone a, another telephone may be inserted in the ground branch at K^1, and it will be found to be loudly affected. If both keys K^1 and K^2 are closed, silence is again obtained in the four tele-

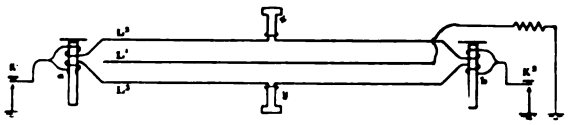


FIG. 7.

phones a, b, x, y. In this case the charge and discharge from the wires L^2 and L^3 divide at the centre and flow back and forth at both ends, x and y being silent because they are at neutral points, and a and b are not affected by the currents which flow through them because of the differential action referred to.

Two systems have now been described, one in which the induced current is lateral, and the other in which it is longitudinal. I think it follows from the foregoing experiments, that where wires are twisted about each other as shown in Fig. 1, both of these actions are combined. At the left hand of Fig. 8, a cross-section of the three wires L^1, L^2 and L^3 [Fig. 1.] is shown. In this position the wires occupy a place with reference to each other exactly as in Fig. 2, and the tendency of the disturbing wire is to cause a longitudinal flow in L^2 and L^3. If repeated cross-sections of these wires are made, a point will be reached at which the three wires are disposed as shown at the right hand of Fig. 8, where it is seen that the disturbing wire L^1 is at an equal distance from L^2 and L^3, and the tendency is to produce a lateral flow. The actual currents produced must be the resultant of these two actions.

Fig. 9 shows a plan quite different in principle from anything heretofore employed. It is of interest not so much on account of any practical application which it may have at present, but because it is a very striking proof of the electrostatic nature of

FIG. 8.

inductive cross-talk between telephone circuits. L^1, L^2 and L^3 are the same wires as used in the previous cases, L^1 being half an inch from L^2, with the addition of an extra wire L^4 placed half an inch

from L^3 and joined by a conductor w with the disturbing wire L^1. L^3 and L^3 are three feet apart. When in this condition the transmitter is operated, no disturbance whatever is heard in the end tele-

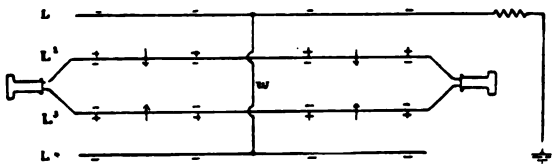


FIG. 9.

phones; if the wire w be disconnected, the usual noise is heard, but is found to disappear as often as L^1 and L^4 are joined together. This action is explained by the fact that L^4 is at the same potential as L^1, on account of being joined to it by the wire w, and acts with the same force on L^3 that L^1 does on L^2. The flow in this case is lateral, as indicated by the arrows; and the telephones are silent.

Neutral points may be produced in a circuit by the use of shunts. Fig. 10 shows the usual arrangement of circuits with the telephones a and b at the ends, and another telephone x, of equal impedance branched between the two wires at the centre. In this case four neutral points are found, two in each wire. The currents produced by the discharge are indicated by the arrows. Thus by the addition of one shunt, the disturbing currents in the end telephones have been reduced one-half. If similar shunts were placed at the quarters, the currents at the end telephones would be still further reduced to one-quarter of their original strength.

This plan is not a practicable one because of its shunting effect on the telephone current, but is of value as showing one of the

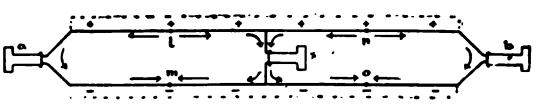


FIG. 10.

actions which occur when instruments are bridged into metallic circuits. It is interesting to note that in Fig. 10 the telephone x is affected by a current twice as great as that which flows through either of the end telephones.

Before closing I shall describe one more experiment. In Fig. 11, L^1 is the disturbing wire and L^2 is a grounded telephone circuit placed one half an inch from L^1. At the centre of L^2 there

is an ordinary telephone repeating coil or transformer c, containing two windings *e* and *f* of copper wire, each having a resistance of 160 ohms. One end of each winding is grounded, and the other end is connected to the line as shown. Assuming that the impedance of each telephone is equal to that of each coil of the transformer, a neutral point will be found at the centre of each half of L^2, and the disturbing currents flowing through the end telephones will be only half as strong as though the transformer were omitted. If now the connections of the transformer be reversed so that the discharges from the two sections of line pass through it in opposite directions, no magnetism will be produced in the core *k*, and consequently the transformer coils offer an easier

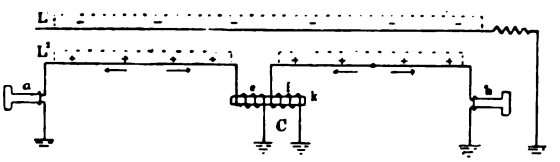


FIG. 11.

path to the discharge. This causes the neutral points to move toward the end telephones and consequently reduces the disturbance still further.

I have not had an opportunity of trying this experiment with a transformer whose coils contained a low copper resistance and a high inductance, but according to theory, we might expect that such a transformer having its coils connected differentially, should free a grounded line of considerable length from cross-talk and other electrostatic disturbances. The number of such coils which can be worked in a given line is of course limited, but with properly designed apparatus a large number of them might be used. This arrangement is also interesting when considered with reference to electromagnetic induction, and brings to mind a question as to whether we may not at some future time abandon the use of metallic circuits and again make use of Steinheil's discovery.

THE PRESIDENT:—A note has been received from Mr Thomas D. Lockwood in discussion of this paper.

THE SECRETARY:—In behalf of Mr. Lockwood I wish to make an explanation in regard to this note which he has sent. He was very anxious to be present at this meeting, but was unable to attend, as he sailed to-day for Jamaica. He has been associated with Mr. Carty and is familiar with his work, and understanding very well what Mr. Carty's ideas were, he wrote this note and sent it to me, expecting to sail last week, but he was detained, so

I sent him the proof Mr. Carty's paper and a proof of his own note, which has been revised accordingly. [The Secretary read Mr. Lockwood's note, as follows:]

Discussion.[1]

[Communicated by Thomas D. Lockwood.]

I understand that Mr. Carty's paper is to be read this month. If in that understanding I am correct, I regret exceedingly that it is not in my power to be present with you in a tangible form. It is moreover difficult to make a written communication upon a topic which as yet is unannounced, but as I have some reason to believe that Mr. Carty will speak upon the subject which he has largely made his own, I will have the temerity to hazard a word or two.

If Mr. Carty continues to hold the views of two years back, I understand him to believe that electrostatic in contradistinction to electromagnetic induction is chargeable with the major amount of discredit as an agency in disturbances on telephone lines; or at any rate, that it is a much more important agent in the development of such disturbance whether noise or cross-talk than has been supposed.

From long experience and observation, it appears to me that the several agencies in causing disturbing phenomena in telephone circuits which manifest themselves in the telephone receiver preponderate largely in accordance with the conditions of the circuit; that the environment (to use the language of social science) of a telephone circuit has a good deal to do with its behavior. Thus a line which has considerable leakage uniformly distributed throughout its length, will suffer very little from electrostatic induction either manifested as a disturbance or as a retarding agency; not because the electrostatic induction is not exercised, or is not present; (or to put in a more modern and I think a more correct phraseology), not because inductive circuits are not formed; but because there being by virtue of the leakage or poor insulation an opportunity for a number of short-cut or cross-conductive circuits, these are utilized for the conduction of the electricity, permitting the establishment of equilibrium or discharge, at the expense of the utilization of the inductive circuits which tend to retain charge. The phenomena exhibited, is comparable to a

1 By Messrs. Lockwood, Kennelly, Anthony, Prescott, Bell, Miner, Watts, Crocker, Upton and R. W. Pope.

gallon vessel, say, in which liquid may be poured; but which has a number of holes bored through its sides. The vessel has a capacity of one gallon, but being leaky does not hold its charge, and the said charge is dissipated as quickly as it is formed. In this case, leakage tends if accentuated, to become the chief adverse agency, as not only does a leaky circuit dissipate induced charges quickly, but it also permits stray currents which may disturb, to come on the lines from external points.

As a matter of fact the several disturbances upon telephone lines are due to a large variety of causes. It is customary to say that the telephone is an extremely sensitive electroscope, or galvanoscope; and that it will take cognizance of almost infinitely small currents. This statement, however, should be made with some reservation.

The more correct statement is of course, that the telephone is a sensitive indicator of *changes* in electrical currents.

A current may be caused to circulate through its helices which is sufficiently strong to make them warm, but if that current be steady, it will not be able to produce a sound in the telephone. Telephone disturbances then, broadly stated, can result only from changes which occur in the electric condition of the circuit within which the telephone is included, and these changes may be produced by conduction from other electrical organizations; by electromagnetic induction, sometimes brought about by changes going on in substantially parallel circuits, and sometimes caused by the mechanical motion of the disturbed circuit itself in a magnetic field; as when a line swings across the magnetic meridian of the earth; and by the successive charges and discharges which cause the alternate completing and disrupting of electrostatic inductive circuits.

If we exhaustively examine the literature of the subject we shall find that the idea that electrostatic phenomena are concerned in disturbances of telephone and telegraph circuits, is by no means new. Mr. Culley presented a paper once upon this subject, before the Society of Telegraph Engineers; which paper together with a most instructive discussion is published in the journal of that society, and Mr. Preece and myself from time to time have also repeatedly referred to it. It is due, however, to Mr. Carty to say that his considerations and researches in this line show him to be an original and independent thinker and observer, and that I have found his work to be instructive in the highest degree.

I think while he is undoubtedly right in attributing a large share of inductive disturbance in telephone lines to electrostatic influences, that he does not give sufficient weight to the equally undoubted fact, that electromagnetic induction is also an important factor; and that as I have already pointed out, the conditions of any individual circuit largely govern its action. An interposed plate or an envelope of metal between the disturbed and disturbing circuits, will, if sufficiently thick, and if united to earth, screen so much of the induction as is electrostatic in its origin; but it will not screen electromagnetic induction; and thus whenever it is worth while the sources of inductive disturbances can be analyzed. And while the inductively neutral metallic circuit,—the circuit with two wires closely adjacent and twisted together,—is indeed a specific against electromagnetic inductive disturbance, it also tends to increase the retarding effect of electrostatic induction exercised between the two wires of the circuit.

Some of the considerations which tend to delay the universal employment of metallic circuits in telephony are, however, that there are an immense number of wires even when but one is used to a circuit; that twice the number of wires means twice the number to get out of order, to cross, break and leak, and consequently twice the number to take care of, and handle.

I hardly think that sufficient thought is given to these matters by many who talk glibly about the necessity and essentiality of using metallic circuits only, for all classes of telephone work.

In conclusion I wish to suggest, that inasmuch as the telephone only notes electrical changes, we cannot accurately say that sounds can be produced in it by electrostatic charge.

It is the fact that it is the motion caused in the act of forming and dissipating charge that produces the disturbing sound. But a charge in the act of formation or dissipation is practically a current; and as a current in the act of change, it necessarily varies also the magnetic field constituted by the circuit within which it is circulating; and as a further consequence therefore sets up in an adjacent and disturbed circuit electromagnetic induction currents. Thus the disturbing action is a compound one; operating first by the successive inductive charges and discharges induced in the disturbed circuit, by the changes occurring in the disturbing circuit; and secondly, by electromagnetically induced currents developed in the disturbed circuit by changes in the magnetic field constituted by the disturbing circuit.

THE PRESIDENT:—Discussion of the paper is now in order. The discussion will be opened by Mr. Kennelly.

MR. A. E. KENNELLY:—Mr. President and gentlemen; the very interesting experiments we have just heard described can leave no doubts in the minds of any of us that electrostatic induction has a very important influence in disturbance between parallel telephonic lines, and assuming that the experiments are truly represented—and I am far from desiring to impugn them—there can be no question that electrostatic capacity is chargeable with all, or nearly all the disturbance that was occasioned in them under those conditions. But the general question of mutual disturbance between telegraphic or telephonic conductors is a very broad and a very vexed one. If two wires run parallel to each other and current flow through one, we must have electromagnetic disturbance. This was one of Faraday's original experiments: and we must also have electrostatic disturbance. The question is, whether we are entitled to calculate upon what proportion of the disturbance is due to one and what to the other. In this case, as in so many others, the question of jurisdiction arises. It is sometimes asked, have we sufficient knowledge of the laws of nature in this particular instance to rely upon the results of calculation, and the answer is, apparently, that we have. Because, however limited our knowledge of the laws of nature may be, it seems to be, in this instance, sufficiently perfect to answer the purposes of the inquiry. Just as when we let a stone fall to the ground, if any one were to ask us if we had the right to predict by calculation what the result of that fall would be, we could say that to a certain degree of accuracy we could certainly do so, by treating the stone as a rigid body, and the laws of rigid bodies are simple and comparatively well understood. But if we had to undertake to say exactly what the conditions would be, then we should have to come to the molecule, and our knowledge of the molecule is at present unfortunately rather indefinite.

The theory of this subject has been treated by several writers, from time to time, and particularly it has been very ably treated by Oliver Heaviside in the Journal of the Society of Electrical Engineers of London, in the February number, 1881.

I have endeavored to obtain some quantitative notion of how much electrostatic disturbance and how much electromagnetic disturbance would take place in Mr. Carty's Figure 2, by assuming certain definite quantitative relations between those circuits. We suppose that L^1 and L^2 are half an inch apart, and all wires are No. 12 B & S gauge copper, also that L^3 is so far remote that its inductive influence on the others and the influence of the others upon it, may be omitted from the calculation. Then it is easy to find out how much electrostatic induction there would be, supposing electromagnetic action absent, and how much electromagnetic action takes place between the two, sup-

posing electrostatic disturbance did not take place. To avoid very great complexities which would take long to follow out to their ultimate consequences, that perhaps is a fair comparison. Under those circumstances, supposing that electrostatic influence existed alone and then afterwards electromagnetic influence existed alone, with four telephones in the secondary circuit, whose resistance, we might suppose, would be 75 ohms each, and whose inductance might be 90 millihenrys apiece, then if the primary circuit had a periodic electromotive force acting upon it, such as would correspond for example to the note A sounded in the telephone at 400, or more accurately, 435 periods per second, it would be necessary to insert an impedance in the primary circuit equal to 400 ohms in order to have electromagnetic action equal to electrostatic action. The fact Mr. Carty has mentioned, that the impedance of the receiving instrument of the primary circuit was high, leads us to suppose that this impedance was much greater, probably ten times as great, and would bear out his statement from the theoretical point of view that the effect of electromagnetic disturbance is virtually null compared with electrostatic disturbance under those conditions. But it would be desirable if Mr. Carty could give us an idea as to how far reducing the impedance in the receiving end disturbed the neutral point and so gave indications of a tendency to equality between the two disturbances.

Of course the rough calculation that I have just presented deals with the subject, ignoring entirely all those waves and oscillations which we know must inevitably pursue their path in primary circuits upon any change of current. We are all familiar with the simple and very interesting experiment of taking the ropes from a flagstaff, say, or the halyards of a mast when they are tolerably heavy—taking them in the hand and giving them a jerk. Of course the ultimate tendency of the displacement of the ends held in the hand would be to cause the rope to assume a slightly different catenary, but the immediate consequence is that a wave travels up the rope, reaches the top, is reflected back and again comes to the hand, subsiding gradually all the time. It may even be sufficiently strong to leave the hand again and repeat its course back to the top. But that represents very faithfully the motion that takes place in any electric circuit when the primary electromotive force is impressed upon it. No matter what the conditions may be of electrostatic induction or electromagnetic induction, so long as both are present, no current can be started in the wire, with any finite change of electromotive force without setting up an oscillation or a wave in that circuit, and those waves run to the end of the circuit if they be not absorbed in the passage, and recoil in the same way that those waves in the rope do. So that if we consider what takes place in any circuit we have to imagine a vast succession of waves which travel one after another, but whose rapidity is so great that they entirely subside within a few

hundredths, at the most, and very often within a few millionths of a second. In the case of Mr. Carty's experiments they probably would not last more than a few millionths of a second There can be no doubt that those waves are there, and even supposing that the mathematical basis on which they rest were entirely destroyed by some fresh discovery, the researches of Hertz have placed those waves on a firm basis. Those oscillations of flow would not probably be a leading factor in this particular instance, but their existence must not be forgotten.

Finally, I think we are much indebted to Mr. Carty for giving us such a clear, succinct and interesting account of experiments which have undoubtedly proved the very great influence of electrostatic induction in the particular case he gives us. I only hope that before closing the paper he will place some more quantitative statements upon the record, by which we shall be able to have a more clear and accurate account of the exact condition of the circuits he experimented upon, and by which we may be able either to check fallacy, to advance theory, or to improve practice.

MR. CARTY:—I go so far as to set forth that the effect of elec-

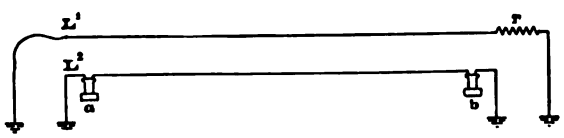


FIG. 12.

tromagnetic induction between parallel telephone wires may be neglected. That is, that when a man is talking on one wire and his speech is heard by induction on a parallel wire, that that speech finds its way between the two wires by virtue of electrostatic induction, and that electromagnetic induction is entirely negligible. I would like to describe a few experiments to prove that; and if they do not prove it I shall be very glad to have that fact pointed out.

I will now show what I consider the most favorable condition for creating electromagnetic induction between two telephone wires. That [indicating T, Fig. 12,] represents the coil of a long distance transmitter sending the most powerful telephone current that it is now possible to generate. L^1 represents the wire connected to earth. It may be, we will say, 200 feet long. Parallel to it and an eighth of an inch away, we will place another telephone circuit. These wires may have an insulation of 10,000 megohms and the resistance might be considered as one ohm. Now a current resulting from a given note in the transmitter through the wire L^1 produces a series of changes in the magnetic field surrounding it, and that action is roughly explained by assuming that the current starting in L^1 induces a current in L^2 in

the opposite direction, and that the induced current will flow through both telephones *a* and *b*. Now, it is a fact that under those conditions absolutely no sound is heard in the telephones *a* or *b*.

That represents the strongest current that it is possible to pro-

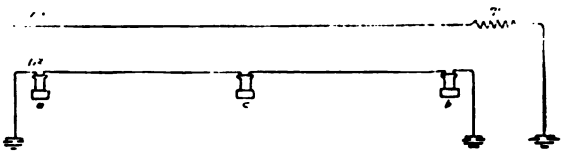


FIG. 13.

duce by that transmitter—the strongest changes—and consequently we should have the greatest fluctuation in the magnetic field surrounding the wire L^1, but under those circumstances there is absolutely no disturbance whatever effected in the telephones *a* or *b* located in L^2. That, I think, is a crucial test.

Now I will describe an experiment which I made with those same wires. We will open the wire L^1 [Fig. 13] at the far end. Then, when the transmitter is operated, noise is immediately heard at the telephones *a* and *b*, located at the ends of the secondary wire, and if a telephone, *c*, be located at the exact centre of impedance it will be found to be silent. When the wire L^1 is opened at the far end we have the maximum electrostatic and the minimum electromagnetic action; when it is closed we have the maximum electromagnetic action and the minimum electrostatic action. The reason that the electrostatic action is slight in Fig. 12 can be seen from Fig. 14. Assuming that *m n* represents the height of potential of the transmitter, the resistance of the circuit is mostly in the transmitter and the fall of potential therein is very sudden. At any point along the wire L^1 the potential would be practically zero, as shown by the dotted line *o p*.

If there is any fallacy in these experiments I should be very glad

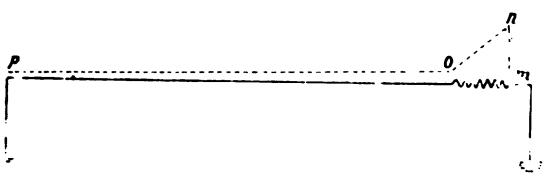


FIG. 14.

to have it pointed out, and if it is admitted that electromagnetic induction is negligible between actual telephone circuits, why I should be glad to have that come out also.

THE PRESIDENT:—The paper is now open for general discussion. We would be glad to hear remarks from any one.

I should like to ask Mr. Carty in regard to this arrangement. He has represented both of those lines as grounded. Why is it not in that case equivalent to connecting the two lines together? In other words, if both lines are grounded why isn't that a complete metallic circuit?

MR. CARTY:—Well, I think Ohm's law will let us out of that. We have both wires connected together by a conductor of zero resistance, and any current generated in this circuit has two paths back to here—one path through the earth, which is zero, and one path through the line, which is of considerable resistance.

THE PRESIDENT:—Do I understand that you consider the earth as a zero resistance in comparison with the two hundred feet of wire?

MR. CARTY:—I have assumed that we have a zero resistance in the earth, and as a matter of fact the experiment, as I have repeatedly tried it, was made using the same earth connection. In an experiment in this room you would probably connect through the water pipe, or there could be a return wire put in there—in that condition, that resistance is zero. As a matter of common practice, if those wires were separated and you talked

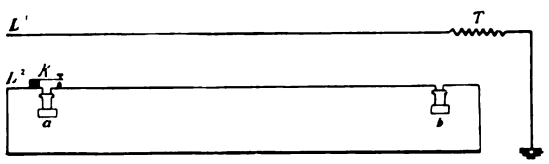


FIG. 15.

on them there would be no interference between them due to earth leakage. I wish to show another experiment in proof of the fact, or what I think is a fact, that electromagnetic induction is negligible when we consider the action that goes on between two telephone circuits. I expressly limit the statement to the action between telephone circuits, and I am not discussing disturbances in general, but merely inductive cross-talk. Now, I will draw a line—L²—[Fig. 15], similar to the one I used before, and we will assume that the circuit is completed by a return wire of no resistance, or of very low resistance, entirely outside of the field of disturbance, so as to eliminate all questions of leakage through the earth. We will say that L¹ is grounded on a gas pipe. We then have the disturbing wire as before, and when the transmitter is operated, loud tones are heard at the end telephones, *a* and *b*, thus: If this were entirely due to the creation of an electromagnetic field around the disturbing wire, the effect on the secondary wire would be increased by short-circuiting one of the telephones by the key *k*. Now it is found that when this is done, instead of increasing the sound in the other telephone, it absolutely removes it. Now, the case just described is another

condition where you should get an increase of electromagnetic action, if the disturbance is due to the magnetic field, but the noise, instead of increasing, disappears entirely. Now, you will see how beautifully the thing may be explained by assuming that the disturbance is due to direct electrostatic action. In this case we will say that the wire L^1, at a given instant, has a plus charge upon it which induces a minus charge on L^2. Now, when the inducing charge is removed the induced charge divides and flows away at both ends. But as there is a great deal of resistance in telephone *b*, and practically no resistance in key *k*, the charge and discharge of L^2 takes place up and down through the key end and none of it goes through the telephone *b*. Now I think that is a complete refutation of the statement that any portion of the noise produced in the telephone is due to electromagnetic action. There is no doubt about it that when the current is going through L^1, in the act of charging it, that there is a magnetic field surrounding that current, but I have shown that it is so feeble that it does not produce any effect in the telephone; that is, the action that is due to the electromagnetic field is so feeble that the ear is not able to tell whether it is present or not.

MR. GEORGE B. PRESCOTT, JR.:—With reference to the first diagram I would like to ask Mr. Carty if he tried the simple experiment of interrupting a battery current in the inducing circuit by means of an ordinary telegraph key?

MR CARTY:—I am very glad that Mr. Prescott brought that up. I have an experiment that furnishes a very good illustration of how electromagnetic action works. I did not try it with an ordinary telegraph key, but I sent through a powerful current so as to get an electromagnetic field with which I could deal. Now in this case T Fig. 12 represents [indicating] a vibrator connected with a large battery. We had a very powerful current flowing through that wire, L^1. The current was constantly vibrating, and it was a very strong current indeed. Now in that case I used the same secondary wire, and a telephone in the centre, and then we found a noise in all three telephones, just exactly what you should expect in dealing with electromagnetic induction. You would expect to find a current at a given instant, with very slight differences, constant in all parts of the circuit; that is, you would expect to find as much noise at the centre telephone as at the end telephone. You would expect that if this middle telephone were short-circuited the noise would be increased at the end telephones, and then you would further expect that if one end telephone were short-circuited that the remaining one would be still louder. That was the case. Now, consider the circuit with the middle telephone cut out and one end telephone cut out, and the other end telephone giving a loud noise, with a strong current in L^1; but with the strongest telephone current we could produce in L^1 there was no noise there at all. And further than that, the methods of every-day

practice have been changed to meet these views, and our predictions and calculations based on this way of working are invariably correct. All of our cables, which are twisted in pairs, are subjected to a very rigid cross-talk test, and some of the refinements which have to be taken into consideration in making those tests are certainly most surprising.

These experiments were begun at Washington, I think, more than two years ago. They have been in the files of various companies and have been reported on by different people, and many of them have been in the Patent Office, and some of them are still in the Patent Office, and if there is a fallacy in the reasoning I am very anxious to find it out, because I have undertaken to point out an error in three authorities to-night, and I would like to withdraw my statements if they are wrong.

MR. PRESCOTT:—Mr. Carty admits that interrupted direct currents in the inducing circuit caused electromagnetic disturbances in the telephone wire. Does not, therefore, his first experiment only indicate that the effects of electromagnetic induction are slight in the case of feeble, rapidly alternating currents, that is telephonic currents?

MR CARTY:—I mean that with the most powerful telephone transmitters in use, with the most powerful telephone currents which I have generated in practice, that the effect of electromagnetic induction is negligible, but that if it were possible to increase the strength of the current I am not prepared to say that electromagnetic induction would not then become a factor. All that I am saying is that for the present it is negligible.

DR. LOUIS BELL:—I think it is a pretty clear case that it takes a good deal stronger current than one would at first suppose to act inductively. I would like, as a matter of curiosity, to ask Mr. Carty if he knows what the numerical coefficient is of self-induction for one of those long-distance transmitters and about what is the largest current that is used in practice. I ask this, simply because it may facilitate computation of the possible effect if one should care to try it. In the case of interference between one telephone circuit and another, I think that unless the lines were most unreasonably close together that the ordinary magnitude of the current employed would not be sufficient to cause disturbance. I have worked it out roughly for heavier currents and the electromagnetic effect is not nearly so great as one would suppose.

MR. CARTY:—I have not the figures in mind, but that matter was investigated by Professor Cross, and he published a paper which can be easily found. I have no figures as to the transmitter coil. I think somewhere in Mr. Kennelly's paper he gives what the inductance of the long-distance transmitter coil is. I do not remember what it is myself.

MR. KENNELLY:—Will Mr. Carty please give us the resistance for that?

MR. CARTY:—160 ohms is the resistance.

MR. KENNELLY:—In that case I beg to point out that the impedance of that would be very considerable, and probably the position Mr. Carty takes is correct. If Mr. Carty can give us some direct experiments made when that impedance is diminished or annulled we can then point out where the magnetic effect will become visible.

MR. CARTY:—I did not quite get what Mr. Kennelly said.

MR. KENNELLY:—If Mr. Carty could give us some experiments in which the impedance in the transmitter was very small we could then readily find at what point the electromagnetic disturbances would become of considerable magnitude compared with electrostatic disturbance.

MR. CARTY:—That would be a question affecting the strength of the primary current, would it?

MR. KENNELLY:—The degree of variation of it, or both—the rate of changing.

MR. CARTY:—Well, I have not the figures, but I produced electromagnetic induction and saw that it behaved as I described. The fact is, that with the transmitters now in use you cannot produce electromagnetic effects in the secondary wire, but just the exact impedance which is necessary to do so with a given receiver, I do not know. I have not had any occasion to determine that and have never done it.

MR. KENNELLY:—Then I would beg to mention that in the rough manner I have indicated the ratio of the electrostatic disturbance to the electromagnetic disturbance as the ratio of the mutual electrostatic capacity to the mutual electromagnetic capacity of the two disturbing wires, per unit of length of either, multiplied by the product of the impedance of the primary, into the impedance of the secondary, divided by two, on account of two telephones. The result is that when you have a large impedance in either one or the other the ratio gets very large.

MR. CARTY:—So that from your statement the results which I described are in accordance with theory as you have stated it.

MR. KENNELLY:—So far as I know, yes.

THE PRESIDENT:—It seems to me that the experiments that Mr. Carty has described here are extremely interesting and very conclusive as to the effects that take place in a particular case. As I understand him, when his line is grounded at the farther end, in which case you will have a series of currents going back and forth upon it, currents as strong as the telephone transmitter is capable of producing, we have no induction whatever—no sound, in other words, in the telephone. And, therefore, electromagnetic induction is so small as to produce no effect that can be heard in the telephone; but when he breaks the ground and leaves the far end of that wire insulated, then operates the transmitter, the wire becomes length plus and minus, rising to a quite high potential, then falling to a low potential. We have at once the effect in both telephones, indicating that the electro-

practice have been changed to meet these views, and our predictions and calculations based on this way of working are invariably correct. All of our cables, which are twisted in pairs, are subjected to a very rigid cross-talk test, and some of the refinements which have to be taken into consideration in making those tests are certainly most surprising.

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static disturbance is something considerable. It seems to me a very interesting fact and a very conclusive form of experiment.

Mr. PRESCOTT:—It hardly seems to me that the experiment is generally conclusive. It merely proves that with the character of current developed by that particular transmitter, the electromagnetic effects on parallel circuits are feeble.

It is easy to imagine a long line with only receiving telephones in circuit that might be greatly affected by electromagnetic disturbances arising from the proximity of direct and alternating dynamo currents, and even ordinary telegraph currents. In fact, in the early days of the telephone, when its use was limited, the latter caused the "frying" noises which were the source of much annoyance.

THE PRESIDENT:—I intended to be understood as applying my remarks to this particular case. Mr. Carty says that when you get your current strong enough, by using a powerful battery and vibrator of rapid period, a very rapid pulsating current, and a very strong one, that he does get an electromagnetic disturbance in this particular case on these short wires.

MR. CARTY:—I would like to put it very clearly before the Institute that my experiments refer only to the disturbance which is produced upon one telephone circuit by induction from another, and that in my experiments I have used the human voice for a disturbing source and covered the entire range of telephonic currents. I have also used a musical note. That is, I have had a boy talking and reading. The original experiments at Washington were tried in the switch-board, and we had relays of linemen reading from the newspapers. At New York I used tuning-forks, because they were more economical.

MR. W. M. MINER:—I would like to ask Mr. Carty if the same result is obtained when the wire that he uses at the top there—L¹—is about three or four times the length of the secondary, and the secondary say a mile in length. The reason I ask this question is, that in the city of Chester, Pa., and about a mile from the exchange—I had called the exchange—when I heard the superintendent at Wilmington, Del., about three miles away, talking with the operator at the Chester exchange; the wires running parallel, and both wires grounded. I spoke to the gentleman at Wilmington, and talked with him as well as I ever talked with any one through a telephone.

MR. CARTY:—I have never tried that experiment, but I have shown the most favorable conditions for developing an electromagnetic field around a telephone wire by the human voice, and the conditions which you name will certainly not be as favorable as the case I described. I predict that you would not get any electro-magnetic induction between those telephone wires that originated in the transmitter. I would say further that all of my experiments have been reproduced, using condensers, which is an interesting point, and the results from condensers were exactly what I got from straight wires.

I wish to point out that there must be some mistake in the reference given by Mr. Lockwood in his note. He says: "Mr. Culley once presented a paper upon this subject before the Society of Telegraph Engineers; which paper, together with a most instructive discussion, is published in the journal of that society, and Mr. Preece and myself from time to time have also repeatedly referred to it." Mr. Culley has not read a paper of any kind before the Society of Telegraph Engineers or Electrical Engineers since 1875. That was before the invention of the telephone. Mr. Culley's paper of 1875 dealt with the induction of telegraph wires. I put in a foot note referring to Mr. Culley's paper in my Electric Club paper. I think that that must be what Mr. Lockwood means. I am very familiar with Mr. Lockwood's writings, but I have never seen anything about that in print. If any one else present has, I think it had better be put on record, as the literature of the subject is meagre at best.

MR. H. FRANKLIN WATTS:—I believe it is considered that electromagnetic induction, pure and simple, is produced by magnetic whirls surrounding the disturbing wire. I would like to ask if it is necessary to have an actual flow of current in the wire in order to produce these magnetic whirls? I am taking it for granted that the magnetic whirls, as I prefer to term them, are the true source of electromagnetic induction. That being the case, is a wire open at one end capable of producing these whirls? If such a wire is *not* capable of producing whirls, then, in the cases to which Mr. Carty refers, in which the wire is open, it is evident that it is due to something else besides electromagnetic induction. I am not able to say whether a wire which is merely charged and in which there is no flow of current will produce magnetic whirls or not.

MR. CARTY:—I think it must be admitted that in the act of charging the wire a current flows, and even although the end be open that current produces a magnetic field, and all of the effects, while it lasts, that would be produced by a current of the same character flowing in a closed metallic circuit. I think that the same thing would hold true of a current actually passing through the dielectric, until the dielectric is strained to its full limit—that even there it sets up a magnetic field. But in my paper I have endeavored to show—or rather in the discussion—that the current required to charge the wire is so small that the magnetic field produced by it is so slight as to be incapable of causing the ordinary current induction or electromagnetic induction in the neighboring wire. If the far end of L' (Fig. 13), were open, but connected to a condenser of one microfarad, then the effect produced on the secondary wire would be practically just the same as though the wire were grounded by a conductor of small resistance. There is a case where the wire when measured for its insulation resistance might have an infinite resistance, and, according to the laws of steady currents, we

could not have a current flowing in it; but, when you consider an alternating current, you know that the current flows in dielectrics until the dielectric is sufficiently strained.

PROF. FRANCIS B. CROCKER:—Mr. President, it appears to me that the question resolves itself into a matter of degree. Mr. Carty states that the ordinary telephone currents, so far as his experiments have gone, fail to show any electromagnetic effect on each other. Now he also tells us that under certain conditions where he had a very much more powerful current with a greater rate of change in current, he succeeded in getting the usual electromagnetic effect that we should expect, and that by cutting out resistance he increased the effect; whereas, in the case of the telephone current, the cutting out of resistance and inductance actually reduced the effect; showing that the facts were not what we generally expect in electromagnetic currents. Now it seems to me that somewhere between these two conditions we must reach a balance point between the electromagnetic and the electrostatic action; in other words, the electrostatic action would equal the electromagnetic action. Mr. Kennelly has suggested how to calculate the value of the induction. It could also be easily determined experimentally, I think. Keeping the line lengths and impedance constant, and varying the rate of change of current, and finding when the strength of the current or rather the rate of its change reaches a certain value, at which the electromagnetic effect is equal to the electrostatic effect. There must really be a limit. As soon as disturbances become audible then they are important. Before that, they are of no importance. Evidently there must be a point somewhere between the two cases given by Mr. Carty at which a sufficiently low impedance, or a sufficiently great electromagnetic effect would give audible electromagnetic inductive disturbances. It is evidently a tangible question. Mr. Carty is right in saying that he considers only the ordinary condition of telephone circuits and telephone "cross talk," and not currents which the ordinary telephone transmitters do not produce. Nevertheless, in order to be sure when the electromagnetic action becomes significant, which might be at a certain current, for example, five times as great as in ordinary telephone circuits, then we should know that ordinary telephone circuits are far below the electromagnetic disturbance limit that is practically appreciable.

MR. FRANCIS R. UPTON:—It seems to me by taking the precautions to do away with the cross talk, that the magnetic induction that might come on to the circuit would be increased; that where the two go oppositely the precautions which would do away with static induction would increase the magnetic induction; what would do away with the magnetic induction would increase the static induction. What I wish to know is, whether these diagrams show anything that is in actual practice in telephone lines.

MR. CARTY:—Five hundred feet was the length of most of the lines used. But all of my practical experience confirms the experiments on these lines, and the system employed to do away with disturbances is effective for both electromagnetic and electrostatic induction. It is much easier to get rid of electromagnetic induction than electrostatic induction. In regard to a point being reached where the electromagnetic induction and the electrostatic induction are neutralized, I would say that I referred to it in the original report which I made on my first experiment—which, by the way, was for the settlement of a practical question involving the payment of a large amount of money—as to whether there was cross-talk or not in a given switch-board. One set of experts would go there and produce what they considered the most favorable conditions for obtaining cross-talk, assuming that it was electromagnetic. Now as I have shown in that case, we would get none at all. So they would go back and report there was no cross-talk. The manager did not know what to do, because he was a witness of the experiment, and he would ascribe it to the weather or some other obscure cause. Another man would go and he might leave some impedance in at the far end of the disturbing wire, and in that case there would be cross-talk. While at Washington I found out what was the matter, and it occurred to me at that time that the two kinds of induction might conflict; that is that a given impulse which would tend to produce a current in one direction would in that case meet one of the electrostatic currents and shove the neutral point one way or the other. The next impulse being in the opposite direction it would move the neutral point the other way. So in looking for the balance, you want to bear in mind that the neutral point is shifting back and forth all the time.

THE SECRETARY:—Before the discussion closes I wish to say in regard to Mr. Carty's paper that he has taken great pains to place these experiments before us, and it appears to me that he has thrown down the gauntlet to a great many eminent authorities, and he has also been so kind as to prepare his paper in advance, so that all who wished could discuss these experiments and pick any flaws which they might see fit in the arguments that he has advanced. In accordance with his suggestion, Mr. Kennelly was invited to open the discussion, believing him to be well qualified to do so. Now when a member undertakes to do a thing of this kind, and bring a paper before perhaps the highest electrical body in this country in order to give others a chance to demolish his arguments, I believe he is entitled to the thanks of the Institute, and I therefore move that a vote of thanks be tendered to him for his able paper.

THE PRESIDENT:—It is moved and seconded that a vote of thanks be tendered Mr. Carty for presenting the paper the way he has before the Institute.

The motion was carried and the meeting adjourned.

[COMMUNICATED TO THE SECRETARY AFTER ADJOURNMENT BY A. E. KENNELLY.]

Note on the relative degree of disturbance caused by electrostatic and electromagnetic induction between certain simple arrangements of telephone circuits.

Referring to Fig. 2 of Mr. Carty's paper, let L_1 be a primary line, L_2 and L_3 the lines of a secondary circuit. Suppose also to simplify the conditions that L_3 is so far remote from L_1 and L_2 by comparison with the half inch of distance separating these latter, that all inductive influence on or from L_3 may be omitted from consideration. The most convenient way of dealing with L_3, is to replace it by the ground, and we have then only to consider the two circuits L_1 and L_2 each taken as negligibly small for the case under discussion.

By considering first the amount of disturbance that would be statically set up in L_2 if electromagnetic action did not exist; and second the magnetic disturbance on the hypothesis that no static induction took place, it is possible to form a comparative estimate of the relative proportions of these disturbances, when, as in practice, both influences act together. The result will not be accurate, because the influence of each not only combines with, but also to a certain extent modifies the action of the other, but the discrepancy due to so elementary a treatment will not in all probability invalidate the results as a first approximation.

Let k be the mutual electrostatic capacity of L_1 and L_2 per centimeter length of each, in A. E. M. U.

Let μ be the mutual electromagnetic capacity, or mutual inductance of L_1 and L_2 per cm. of each in A. E. M. U.

Let l be the length of each of the wires in cms.

Let e be the primary circuit E. M. F., which is periodic in value.

Let n be the number of periods completed by e per second.

Let ω_1 be the impedance in the circuit of L_1 in A. E. M. U.

Let ω_2 be the impedance in the circuit of L_2 in A. E. M. U.

Let γ_e be the current in each of the two end telephones of the secondary circuit owing to electrostatic induction when electromagnetic induction is supposed to be absent.

Let γ_m be the current in each of three telephones in the secondary circuit (one at the centre of L_2 and the others at its ends), when electromagnetic action is supposed to exist between the circuits to the exclusion of static induction.

Let q be the total electrostatic charge induced on L at any moment by static induction.

Let r be the radius of each wire (copper), in cms.

Let the wires be parallel and the distance between their axes be d cms. in the horizontal plane elevated h cms. above the ground.

Then if the end of L_1 distant from the transmitter be disconnected from the ground and electrostatic influence only be supposed to act upon the secondary circuit, the potential of L_1 at any moment will be e, the value of the periodic E. M. F. while the induced charge upon L_2 will be expressed by

$$q = -e\,l\,k.$$

The flow of current from L_2 to ground will at any moment be the time rate of change in q or numerically

$$\frac{dq}{dt} = l\,k\,\frac{de}{dt}.$$

and since this by symmetry will divide equally between the two end telephones in L_2, the current in each is numerically

$$\gamma_e = \frac{lk}{2}\frac{de}{dt}$$

Now with electromagnetic induction active, and static action suspended, the distant end of L_1 is connected to ground through a certain impedance. The current then existing in the primary circuit at any moment is

$$\frac{e}{\omega_1}$$

and in the secondary circuit $\frac{l\,\mu}{\omega_1}\frac{de}{dt}\cdot\frac{1}{\omega_2} = \gamma_m$ since this affects all three telephones alike.

Since γ_e and γ_m are periodic currents of the same frequency, they will produce equally loud sounds in the telephones when of equal numerical value, and the ratio of static to magnetic disturbance in the two cases will be

$$\frac{\gamma_e}{\gamma_m} = \frac{k}{\mu}\,\frac{\omega_1\,\omega_2}{2}$$

Provided then, that the impedance of the primary circuit at the receiving end is always large, the ratio of static to magnetic disturbance will be approximately expressed by this equation. In order to apply it to the case of Fig. 2, we have to determine from the geometrical relations of the wires their mutual capacities k and μ.

The value of k in A. E. S. U. has been given by Mr. Oliver Heaviside in the Journal of the Society of Electrical Engineers (London) for April, 1881, (with a misprint in the formula here avoided)

$$k = \frac{\frac{1}{2} \log \varepsilon \frac{\sqrt{d^2 + 4h^2}}{d}}{\left\{ \log \varepsilon \frac{2h}{r} \right\}^2 - \left\{ \log \varepsilon \frac{\sqrt{d^2 + 4h^2}}{d} \right\}^2}$$

Substituting for d 1.25 (cms. or half an inch.)
for h 1000 (cms. or about 33 feet.)
for r 0.1 (cms. or 0.04″ about representing No. 12 B. & S. gauge.)

$k = 0.0714$ A. E. S. U. $= 7.94 \times 10^{-23}$ A. E. M. U. or 0.0128 microfarads per mile.

The value for μ can also be found from another formula first published by Mr. Heaviside in the Journal of the above mentioned society for May, 1878, namely:

$$\mu = \log \varepsilon \frac{d^2 + 4h^2}{d^2}$$

from which $\mu = 14.75$ A. E. M. U. or 2.37 millihenrys per mile.

Thus for equality between γ_c and γ_m we must have with these values of k and μ

$$\omega_1 \omega_2 = 3.717 \times 10^{23} \text{ A. E. M. U.}$$

Three telephones of 75 ω and 90 h each would to a frequency of $n = 435$ corresponding to the note a^1 offer an impedance of $\omega_2 = 7.72 \times 10^{11}$ or 772 ohms, the impedance of the three telephones to this frequency being more than three times their resistance.

For $\gamma_c/\gamma_m = 1$, ω_1 must now be $3.717 \times 10^{23}/7.72 \times 10^{11}$ or 4.816×10^{11} or 481.6 ohms in round numbers 500, while with the primary circuit of Fig. 2 having a transmitter of 165 ω and 730 h with a call-bell at the distant end of 550 ω and 2.7 H, the total resistance would be 715 ω, the total inductance 3.43 H, and the impedance to the same frequency of $n = 435$ would be $9.40 \times 10^{12} = 9400$ ω or about thirteen times greater than the total resistance. The impedance ω_1 is thus nearly twenty times greater than that needed by the formula for equality between γ_c and γ_m and roughly the static disturbance in Fig. 2 may be estimated as twenty times greater than the magnetic.

A paper read at the 56th Meeting of the American Institute of Electrical Engineers, New York, April 21st, 1891. President W. A. Anthony in the Chair.

ELECTRICITY IN THE PRODUCTION OF ALUMINIUM.

BY ALEXANDER S. BROWN.

The glamour that has surrounded this metal since 1856, when H. St. Claire Deville first obtained it in sufficient quantities to study its physical and chemical characteristics, and the almost monthly presentation before one or another of our scientific societies of papers treating of its properties, etc., makes it a most difficult subject to handle with originality. Scientific and newspaper eulogists have expended a great deal of time and many words in extolling the unusual combination of useful properties which it possesses and have given their imaginations full play in describing its future triumphs in the metallurgical world; but as yet, little has been actually accomplished in the way of accurately determining the commercial status and mechanical value of this metal compared to others more familiar to us. While it must be confessed that in certain respects its merits seem such as to warrant great expectations, those interested in its production and its commercial success have experienced not only the usual difficulties inseparable from the introduction of any new metal, but many others due to the too great esteem which the public has been led to bestow upon it. When sufficient knowledge of its characteristics has been acquired to form an unbiassed judgment as to its true rank in the metallurgical world, there is little doubt it will be found to be one of the most useful metals ever discovered, and that it will not only largely displace copper, but will make for itself new and important fields of usefulness.

Still, of all the metals commercially familiar to us, aluminium has unquestionably made the most rapid progress. The long and

tedious development of iron, steel, copper, tin, lead, zinc, etc., extending over centuries and centuries, is familiar to us all; the fact that this new metal (only discovered in 1827, and extracted in perceptible quantities not earlier than 1854) has already entered into commercial competition with the oldest and most useful metals, substantially at their own prices, would alone make aluminium a most interesting subject of study and speculation.

In the year 1760, or thereabouts, a chemist named Morveau, by calcining alum obtained a substance which he called alumina. Shortly thereafter Lavoisier suggested the existence of metallic bases in the earths and alkalies, and, naturally, alumina was immediately suspected of being a metallic oxide. Some interesting speculations were indulged in on this subject and this hypothetical metal received the name of aluminium.

As early as 1807, Davy and others tried ineffectually to decompose alumina by various means, including the use of the electric current. A chemist named Oerstedt thought he had produced the metal in 1824, but a German chemist named Wohler was the first to actually demonstrate its existence. He extracted the metal in 1827 by decomposing the chloride of aluminium by means of metallic potassium; 20 years later he also reduced it with metallic sodium. The first commercial product was exhibited in 1854 by H. St. Claire Deville, a French chemist, whose researches as to its chemical and physical properties (fully reported in the French journals of the day), were so thoroughly exhaustive that his writings are still standard authority on the subject. He used several methods, the most successful of which was reduction by sodium. Patronized by Napoleon the Third, who entertained great expectations concerning the use of this metal in war equipment, Deville established commercial works near Paris, which, after varying fortunes, were finally permanently located at Salindres, France, and these works for many years produced substantially all the pure aluminium consumed in the world. It may interest you to know that up to 1886 the annual production of this metal was less than 10,000 lbs., of which 6,000 to 8,000 lbs. were produced in France. In 1860 it sold at over $90 per lb. In 1887 it was sold at $5 per lb. and is now selling at $1 for a commercially pure article—this term signifying about 97 per cent. purity in the metal. It is known that by processes now in operation, the metal can be

produced in large quantities at a price which will put it on a par with copper, bulk for bulk.

Until the dynamo came into commercial use, the production and development of aluminium was almost entirely along chemical lines. It is a metal that possesses remarkable affinity for oxygen and chlorine, and, so far as I know, only potassium and sodium are capable of extracting oxygen and chlorine from it on a commercial scale. Naturally, therefore, the history of its progress is interwoven with that of the commercial manufacture of sodium and potassium. As early as 1856, one Mons. Alfred Monier, of Camden, N. J., claimed to have successfully made sodium by a continuous process, as a preliminary step in the manufacture of aluminium, and, later, he exhibited several small specimens at the Franklin Institute. This is undoubtedly the earliest record of the production of aluminium in this country.

Three years later, in 1859, the first aluminium works in England were started at Battersea, near London, but no details concerning them are obtainable. In 1860 other works were successfully established at Newcastle-on-Tyne, by Bell Bros., which were operated with more or less success until 1874. These gentlemen seem to have been close investigators of the subject, and have published important information concerning the characteristics of this metal. Other spasmodic attempts to establish aluminium works were made in Germany and other countries, but it may be truthfully said that France, which created that industry, remained its home until the development of the electrical processes which have now superseded all others. The most important effort on this line was that of Mr. James Webster, who, in 1882, re-established the aluminium industry in England, forming the Aluminium Crown Metal Company at Hollywood, near Birmingham. The principal features of his patent, which this company operated, were the large cheapening of the manufacture of alumina (Al_2O_3.) He claimed to obtain a product containing about eighty-four per cent. of alumina—a substantial gain upon other known processes—and also to save half the cost of manufacture by utilizing by-products. In fact this company was for several years a successful competitor in the English markets to the French works at Salindres, though its output was very limited.

About 1886, a young Philadelphian—Mr. H. Y. Castner—invented and secured a number of patents to cover an improved and economical method of making sodium, which he claimed re-

duced its cost to twenty-five cents per pound. This invention he took abroad and in 1887 the Aluminium Crown Metal Company was re-organized under the name of the Aluminium Company, Limited, and extensive works for the manufacture of sodium, primarily, and of aluminium, secondarily, were erected at Oldbury, near Birmingham. They were completed in July, 1888, and were alleged to have a very large capacity for producing aluminium. The reduction of aluminium at these works was conducted strictly on the lines of Deville's process—Castner's improvements being solely in the cheapening of sodium—and came to be known as the Deville-Castner process. Considerable quantities of metal were produced at these works and most of it was of exceptional purity. As long as aluminium ruled above $3.00 a pound, they were commercially sucessful; but inasmuch as it takes nearly three pounds of sodium to make one of aluminium and the various other steps in the reduction are rather expensive, this process has not been able to compete commercially with the less costly electrical processes, and the Oldbury works are now devoted exclusively to the manufacture of metallic sodium, which finds profitable markets in Germany and elsewhere, being largely used in the chemical manufacture of anti-pyrine

During the development of the various sodium processes to which I have above referred to, the possibility of utilizing electricity was not neglected. Among the earliest patentees in this field, were Camille A. Faure, in 1880-4, and R. Gratzel in 1883. Other inventors before those dates realized the value of electricity in smelting and other electro-chemical processes, but they were the first to give practical direction to theoretical speculation. Nothing has recently been heard of Faure's process, but that patented by Gratzel in Germany, in 1883, was practically utilized by a firm at Hemelingen, near Bremen, called the Aluminium and Magnesium Fabrik. The Gratzel process consisted, substantially, in electrolyzing a bath of fused chloride or fluoride of aluminium by means of anodes composed of a mixture of carbon and alumina, the current being furnished by a dynamo, but for some reason it was abandoned by the company, in 1887, for another process, the nature of which has been kept a profound secret.

Between 1883 and 1886 a number of inventors secured patents for electrical processes, but I need only mention those which, in the struggle for the survival of the fittest, have come into commercial prominence.

On April 2nd, 1889, Mr. Charles M. Hall, of Oberlin, Ohio, secured patents for a process of reducing pure aluminium In this process alumina is dissolved in a fluid bath composed of aluminium fluoride and potassium fluoride, with, perhaps, an addition of lithium fluoride, this bath then being electrolyzed by means of an electric current conveyed to a carbon-lined furnace through an anode of some non-carbonaceous material. In electrolyzing this bath, the containing vessel is made to serve the purpose of the negative electrode. In another patent Mr. Hall claims the use of a bath composed of alumina dissolved in compound fluorides of aluminium with alkaline earthy metals, and in still another patent, the use of a bath composed of alumina dissolved in the fluorides of aluminium, calcium and sodium; these materials being obtained by melting together aluminium fluoride, fluorspar and kyrolith With this latter bath a carbon anode can be used without the bath being affected by its disintegration.

The Hall group of patents is now controlled by the Pittsburgh Reduction Co., of Pittsburgh, Pa., which has had its metal on the market since March, 1889. The plant has a daily capacity for producing about 300 pounds of metal, the selling price of which was $2.00 per pound, until the early part of March last, when it was reduced to $1.00 per pound for commercially pure aluminium, of a guaranteed purity of ninety-seven per cent. The reduction in price, considered in conjunction with the limited amount of pure metal produced, would appear to confirm the generally received impression that while the pure metal is specially adapted to the manufacture of articles of art and ornament, it is not displacing tin, platinum, silver, etc., to anything like the extent its admirers have supposed it would. The fact is, aluminium as a pure metal is not destined largely to supplant other metals in the arts and manufactures; it is to its alloys that we must look to redeem the inflated reputation of the pure metal. The true value and use of aluminium seems to have been forecast by Deville, who predicted for it an intermediate position between the precious and baser metals, in which relation to manufactures he said it was destined to fill a long-felt and important want He doubtless referred to the pure metal, but, curiously enough, his prediction applies with greater force to its alloys, which have been demonstrated by actual use and thorough tests to possess physical and chemical properties that place them

above all other alloys of copper, iron, and in some instances even of steel.

The electric smelting process of the Messrs. Cowles of Cleveland, Ohio, was the first to secure a commercial footing in this country. These gentlemen have taken out a series of patents covering their process, and they have exhibited very commendable energy and perseverance in establishing a market for their products. Both the process itself and apparatus used have been so frequently and minutely described that they are doubtless familiar to all of you and a recapitulation of its details in this paper would be superfluous. As defined in numerous papers and publications emanating from, or inspired by the inventors, the process may be described as the reduction of metals by smelting their ores with heat produced by the passage of an electric current through broken resistance material—that is, in the presence of an incandescent material, (carbon being generally preferred.) Stress is laid on the alleged fact that the reduction is purely and solely a smelting operation, due to a heat far greater than can be obtained by any agency other than the electric current. The electrical (chemical) effect of the current is distinctly disclaimed in their patents and publications. Perhaps I may not unprofitably consume a part of your time by explaining that the furnaces used at the company's extensive works at Lockport, are rectangular and oblong in shape, comparatively shallow, and are composed exteriorly of walls of fire-brick or metal, lined with pulverized charcoal or other form of carbon or similar material, of electrical conductivity inferior to the ore to be smelted; this material being generally previously saturated with lime-water to increase its resistance to heat. At each end of these furnaces are inserted rods of carbon which, passing through the walls and linings, are brought very nearly into contact within the body of the furnace, and, being connected to suitable wires or cables coupled to the dynamo, constitute the positive and negative electrodes. The charge consists, ordinarily, of a mixture (in different proportions) of an electrically resisting material—such as electric light carbon—and the ore to be reduced; though (in cases where the ore itself has sufficient electric resistance) the carbon is sometimes dispensed with. The charge fills the furnace and presses against the sides and ends of the electrodes which project into the furnace. When a current of electricity is passed, the electrodes are so adjusted as to form an

arc, and the current then flows from the sides and ends of the electrodes through the broken resistance material, which, of course, becomes incandescent, and the intense heat developed by this incandescence reduces the ore; the metal flowing to the bottom of the crucible and being subsequently tapped or dipped out when the charge is consumed. The gases liberated in the operation pass out through openings in the fire-brick cover of the furnace. From the character of the apparatus and manufacture or operation described, it will be obvious to you that the reduction of the metal is (as claimed by the Cowles Company) accomplished by means of heat generated by the passage of an electric current through a mass of high resistance material, and that if the chemical effect of the current assists the reduction at all, such utilization is entirely accidental and unsought. Neither aluminium nor any other metal is produced on a commercial scale by this process *as pure metal.*

The principal rival to the Cowles process for producing aluminium alloys is the Heroult process, which reduces aluminium and other metals from their refractory ores in the presence of another alloying metal and is contradistinguished from the Cowles process mainly by the fact that it operates exclusively—or at least almost entirely—by utilizing the chemical or electrolyzing action of the current. This process is the invention of a young French electrical engineer named Paul L. T. Heroult, whose affiliations with the firm of Messrs. Henry Merle & Co., owning and operating the Deville Aluminium Works at Salindres, and his subsequent studies at the Polytechnic Schools at Paris and Switzerland, first directed his attention to the electrical reduction of aluminium. In 1886-7 he secured French, English and other European patents, covering several processes for the extraction of aluminium, silicon, boron, etc., from their ores, both as pure metals and in the form of alloys. On August 14th, 1888, he secured a United States patent No. 387,876 for the process of preparing aluminium bronzes and their alloys, and it is understood has pending in the Patent Office a series of applications covering other parts and features of his inventions. Though the Heroult processes have been very little discussed or even described in print, and are therefore almost wholly unknown to the public, their commercial success abroad has been far greater than has attended any other aluminium process, and while those owning the American patents seem as yet to have done little in

the way of exploiting them, the success of a small plant put into operation at Boonton, N. J., for private demonstration purposes, in April, 1890, has been such as to give it rank among the prominent aluminium processes in this country. Having had exceptional facilities for studying the workings of this plant during the past four months, I might be pardoned—especially as so little is publicly known of it—for describing it at length, but as I particularly desire in this paper to direct your attention to the electrical apparatus used at Boonton, by way of illustrating the great power and value of the electric current in metallurgical operations, it will suffice here, to say that the patents of this young engineer, having first been acquired by Swiss capitalists, were afterwards bought by the celebrated syndicate which, under the name of the Allegemeinen Electricitats Gerellschaft of Berlin, substantially controls the entire electrical field in Germany. This syndicate, after carefully examining all other processes then known, invested ten millions of francs in the Heroult process and are operating very extensive works, which obtain their power from the falls of the Rhine, at Schaffhausen. There they produce not only pure aluminium and all its alloys, but also alloys of silicon and other metals. Those works are supplied partly with dynamos of the type I wish especially to describe to you, and partly with dynamos of 600 electrical horse-power, a complete description of which I regret I am not able to place before you at this time, as they would interest you. These machines weighing over fifty tons each, revolve horizontally; they have 24 pole-pieces with collectors over 12 feet in diameter, and were especially designed for economical work.

To give you a comprehensive idea of the method employed by Mr. Heroult in manufacturing aluminium and its alloys—and so far as electrical apparatus is concerned, there is little difference between his and the Cowles process—it will be best to describe the electrical plant and crucible separately.

In the electrical apparatus the most important feature, of course, is the dynamo which generates the current. This machine was designed by Mr. C. E. L. Brown of the Oerlikon Works, Zurich, specially for electro-metallurgical work and is the only one of its kind in America. It weighs, complete, over 19 tons and when driven at 220 revolutions generates a current of 35 volts and 3,500 amperes. The general appearance and design of the machine is very well shown in the following illustration

(Fig. 1.) It is, as you will observe, a six pole machine: having an armature of the Gramme ring type, and the current is taken off commutators at either end, by 72 brushes. The armature is 39 inches in diameter, and the shaft, (including its clutch,) is 11 feet 7 inches long. The commutators are each 19 inches in diameter and 29 inches long; each commutator containing 60 segments weighing 13 lbs. apiece, or a total of 780 lbs. for the

FIG. 1. - Oerlikon Dynamo at Boonton, N. J.

segments for one commutator alone. The core of the armature consists of a very large number of sheet iron discs clamped together in a frame of aluminium-bronze. About $\frac{3}{8}$ of an inch from the outside edge of the core, 120 holes extend from one end to the other, through each of which passes a copper wire $\frac{1}{2}$ inch in diameter. These wires, which of course are thoroughly insulated

from the core, form part of the winding. The remainder of the winding is composed of copper bars ¼ of an inch think, 31 inches long and 2¼ inches wide. The shape of these bars and the interior appearance of the armature can readily be seen in the illustration (Fig. 2). This photograph was taken about 6 months ago, while the armature was being repaired in consequence of an accident. Though, as previously mentioned, the works at the Rhine Falls

FIG. 2.—Armature of Oerlikon Dynamo.

are in part equipped with much larger dynamos, of 600 electrical horse-power, it has been found that they do not give nearly as economic results as the other dynamos of the kind I have attempted to describe. This latter type is also in universal use at Mr. Heroult's own works at Froges, France. At the time a friend visited the Schaffhausen works last summer, one of these 150 horse-power machines had been running without a moment's

intermission five months, and another nearly four months. Though rated at 150 horse-power by the Swiss makers, the same machine in this country would be rated at over 200 horse-power, and would be driven at much greater velocity. In actual work they are frequently run up to 300 revolutions, and though it sometimes happens in practical operations that heavy short circuits occur, there is hardly any perceptible sparking at the brushes, even when the current rises to 8,000 or more amperes. The indicator attached to two of these machines (coupled in series) at the Rhine Falls has frequently recorded 25,000 amperes without any evidence of the short-circuiting appearing at the brushes.

At the Rhine Falls, at Froges, and at Boonton, these machines are coupled directly to turbine wheels arranged upon horizontal shafts. The frame to which the field-magnets are attached is a solid casting, weighing alone over 9 tons. The field-magnets are excited by a smaller dynamo driven by belting from a pulley on the main shaft. This smaller machine was also designed by Mr. C. E. L. Brown; the armature being similar to that of the large machine, except that it has but one commutator. This exciting machine is run at 60 volts and 120 amperes, and its performance is worthy of all praise.

At Boonton the 'bus wires are 8 in number; 4 positive and 4 negative; each wire being square and having a cross-section of one inch. They are, in fact, rolled copper bars, uninsulated, because the low potential of the current requires no special insulation. An ammeter is placed in the main circuit by a simple arrangement, for the guidance of the workman who specially looks after the crucible. The distance from the dynamo to the furnace, (which is situated in another room), is only 14 feet. The 4 positive wires pass directly to the clutch which holds the anode, and the 4 negative wires pass from the ammeter back to the dynamo.

The crucible (of which Fig. 3 gives a good representation, together with the method now in use for raising and lowering the anode) is composed of a strong iron box which has a deep lining of carbon, previously ground very fine, mixed with sufficient tar and thoroughly compacted on the bottom and around the sides of the containing iron box. After the furnace is thus re-lined, the carbon is baked in order to drive away the tar and harden the lining. You will see that the position of this crucible is directly the reverse of those used by the Cowles Company,

and that the carbon crucible itself actually forms part of the electric circuit. Unlike the Cowles process, Heroult's is a continuous process; the reduced metal being tapped from the bottom of the crucible while the machine is merely stopped or slowed down for a few minutes. The interior dimensions

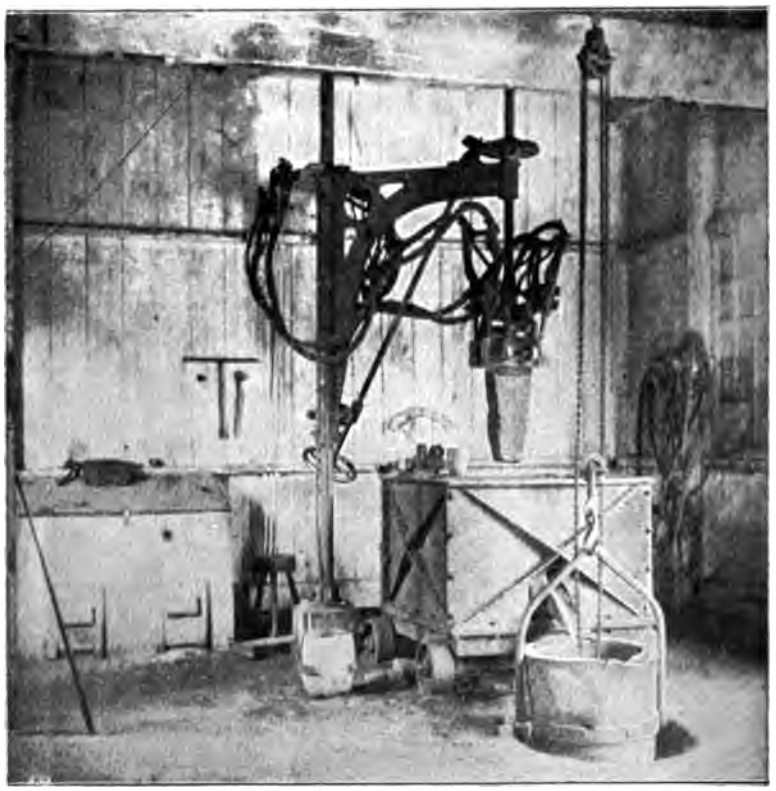

FIG. 3.—A Heroult Crucible used in Aluminium Extraction.

of the crucible vary with the ore to be reduced, and the size and character of the anode used. Ordinarily it has an interior depth of about 22 inches and a clear space of 6 inches between the walls and exterior surface of the anode. The electric current passes into the crucible through the suspended carbon anode, the vertical position of which is controlled by an

attendant who lowers it from time to time to preserve an approximately constant distance between the bottom of the anode and the surface of the molten bath in the crucible. The anode usually dips several inches into this molten bath and in practice the intervening space between its bottom and the surface of the molten metal, is kept at about an inch by the attendant, who by watching the needle of the ammeter is enabled to maintain the electrode in its proper relative position without difficulty. In order to get as perfect a contact as possible, between the furnace and the cables leading therefrom back to the dynamo, the cables are firmly clamped to a large block of zinc, which projects from the rear of the furnace and is kept cool by water circulating in pipes around which the zinc is cast. While, properly speaking, the crucible has no top, as a rule plates of carbon imbedded in dry alumina spread over the top of the crucible, are so adjusted as to fit close to the anode in order to retain most of the heat. The mouth of the crucible is formed of slabs of carbon built up funnel shape. The hearth or floor is pierced by a tap hole, which is kept closed by a plug of carbon or fire clay, and is opened from time to time to allow the molten metal to run into a carbon lined ladle, from which it is cast into ingots.

There are a few distinctive but not important differences between Heroult's method of making alloys and reducing pure metals simply. In making alloys, the base alloying metal is first melted in the crucible by the current, or molten metal is poured into the furnace, to form a liquid metallic cathode. This is usually simply and economically accomplished by throwing pieces of the metal—copper for instance—into the furnace, seating the anode upon them and passing the current. At first the current is very unsteady, arcs constantly forming between the copper and the anode, but when the copper is entirely melted (and it takes but a very few minutes to melt 200 or 300 pounds) the ore to be reduced is added in sufficient quantities to form a bath, and floats on top of the molten copper. The anode having been raised so as to leave a space of about an inch between its bottom and the molten copper, the current becomes steady, and the operation is established. Thereafter the ore is introduced in small quantities, at frequent and regular intervals, until the desired percentages of aluminium or silicon contained in the alloy have been secured. This percentage can be regulated entirely at will up to about seventy per cent. of reduced metal. No exter-

nal heat is used, and the process is absolutely continuous at the will of the operatives, which are special features of the Heroult system. The passage of the electric current through the bath, serves not only to keep the copper in a fluid state and to melt the ore, but also electrolyzes and decomposes the latter. It is a point of careful regulation in the Heroult processes to maintain only sufficient heat to keep the bath fluid, since any further expenditure is not only useless but lessens the output. The oxygen from the reduced metal combining with the carbon of which the anode is composed is burnt with the production of carbonic oxide gas, which escapes from the crucible around the sides of the anode and at the feed hole. The product is withdrawn at intervals by tapping the bottom of the crucible as in ordinary smelting. Each time the metal is tapped, a sample is carefully preserved for accurate analysis, to determine the percentage of contained aluminium.

From a metallurgical point of view, the amount of labor required to operate this process is astonishingly small. The metal produced in the Heroult furnace is sold generally in ingot form, just as it is tapped, without re-melting or being subjected to any purifying manipulation. It is always, however, very pure, sometimes dropping as low as ninety-eight and one-half, but generally running over ninety nine per cent. of absolute purity. This is largely due to the great care exercised in selecting raw materials. In making bronzes, the best Lake Superior copper is used, while the oxide from which the aluminium is reduced is an artificial product, manufactured in Prussia. The number of materials from which aluminium may be reduced by these processes is very large, but the strong influence exerted upon this metal by impurities reduces those ores commercially available in this country to corundum and beauxite, both of which are largely used in making low grade metal for treating iron and steel. Kryolith—natural or artificial—is another mineral that plays an important part in the commercial production of pure aluminium, since it is the best flux for alumina, the ore from which the metal is generally reduced. There is to a certain extent a diversity of opinion in regard to the value and properties of aluminium, which is partly the natural sequence to the illusions and exaggerated opinions concerning its commercial and practical value, resulting from the romantic descriptions, speculations and suggestions, with which our scientific and trade literature have been filled ever since De-

villes's researches were published, and partly the result of impurities in the metal as ordinarily produced. For thirty-five years after Deville first produced it, aluminium, though much talked of, was but little seen outside of laboratories and lecture rooms, except as we find it on the French and English markets manufactured into ornamental shapes. It is only since 1886 that it has been manufactured in sufficient quantities and at sufficiently low prices to permit practical metal workers to test its properties on an adequate scale The very unusual qualities which give it merit, and the great depreciation it undergoes when only slightly contaminated with foreign impurities, such as iron and silicon, together with the further fact that it has only been recently commercially produced of ninety-nine per cent. purity, have had a tendency to prejudice this metal in the eyes of practical people. From this depression it will slowly recover as they secure pure metal. In fact this very condition was forseen by Deville, who very early pointed out that those handling aluminium and its alloys would be disappointed at the poor results they obtained where they used metal containing more than one per cent. of impurity.

Pure aluminium has a beautiful white color, slightly tinged with blue. It is somewhat fibrous in structure and bends considerably before breaking. If the metal contains more than one per cent. of impurity it becomes brittle and has a crystalline structure. When pure, it is soft enough to be easily cut with a knife, while the addition of a few per cent. of impurities makes it exceedingly hard. It melts at a slightly lower temperature than silver, and has a specific gravity of 2.6. Taking aluminium at 1.0 the following table (from Deville), will be of interest, as showing the comparative weights of the different metals.

Platinum	8.6
Gold	7.7
Lead	4.8
Mercury	4.2
Copper	3.6
Iron	2.9
Tin	2.8
Zinc	2.8
Aluminium	1.0

The pure metal takes a beautiful polish, uniting with oxygen slowly and with great difficulty, even at a red heat; and when (from long exposure), it finally acquires a very thin coating of oxide, it is not perceptibly tarnished. This coating of oxide protects it from further atmospheric or other baneful influences.

It is well known that pure aluminium can be drawn out into the finest wire or into tubes; rolled into plates, stamped into medals (similar to those distributed at the recent Electric Light Convention) beaten into the finest foil, etc. It has an elasticity similar to that of silver, and a tensile strength of about 26,800 pounds per square inch. The addition of 6 per cent. of copper, however, increases the tensile strength to that of wrought-iron, and a slight percentage of silicon raises its strength to 90,000, though at the expense of other qualities.

The compounds of aluminium formed by the addition of copper, brass or iron, are unquestionably the most commercially important of the aluminium alloys. Aluminium combines with copper in all proportions, but its alloys are as easily influenced by impurities as is the pure metal. It is an important fact worth noting that, when alloys are made by mechanically mixing aluminium and copper, iron, etc., the product must be re-melted a number of times to secure any uniformity in the texture of the alloy. The finest alloys, by far, are those made by uniting the aluminium to the basic metal in atomic proportions, as it is reduced, as in the Heroult process, the uniformity of whose alloys is remarkable. Probably the most useful compound of aluminium and copper is what is known as 10 per cent. bronze, containing 10 per cent. aluminium and 90 per cent. of copper. This alloy is an almost perfect imitation of gold in color, and is susceptible of taking and retaining a most beautiful polish. It can be rolled, or drawn, or cast very readily, but in casting, the bronze shrinks considerably, and care must be taken to have the feeders amply large. The tensile strength of this bronze is enormous, and the following table from "Trautwine's Engineer's Pocket Book" shows it in comparison with other metals:

	Tensile Strength.
Cast brass	23,000
Annealed brass wire	49,000
Cast copper	24,000
Copper bolts	33,000
Annealed copper wire	32,000
Gun bronze of copper and tin (cast)	39,000
Cast-iron gun metal, U. S. Ordnance	30,000
Average American cast-iron	16,000
Good wrought-iron	50,000
Best American wrought-iron exceptional	76,000
Iron wires, rope	38,000
Malleable iron castings	48,000
Steel plates (rolled)	81,000
Cast steel, average Bessemer ingots	63,000
Aluminium bronze, in castings, over	100,000
Aluminium bronze "special" in castings up to	130,000

[COMMUNICATED AFTER ADJOURNMENT BY PROF. M. G. FARMER.]

During the year 1859, while I was experimenting upon wires of different materials for the purpose of producing an electric light, I chanced to try a wire of aluminium. When the wire was well heated by the electric current, I noticed that the light given out by it approached quite closely in brilliancy to that given out by incandescent platinum. When I approached a steel darning needle to the wire loop I saw that the wire was deflected, as any wire should be which carried an electric current in a magnetic field. Its behavior was different in this respect: a wire of platinum would retain much of its elasticity even when heated to a high temperature; not so with the wire of aluminium, it seemed to wholly lose its elasticity, and was as mobile or flexible as a string or thread. I do not know what, but something prompted me to puncture the heated aluminium wire, and when I withdrew the darning needle there followed it a stream of melted aluminium to a distance as great as one quarter of an inch or more. I swung the loop but the wire did not fall apart, although it was fluid internally, but exteriorly it seemed coated with an oxide which retained its cohesion and supported the wire against gravity. I noticed that the wire after it had been heated and cooled once or twice, was shrivelled and appeared shrunken like to a shed snake skin.

My next experiment of promise was in 1856, when I deposited electrolytically some aluminium upon a copper wire; I also coated a german-silver spoon with it and the plating was well done and it wore well. This experiment led me to predict at that time that at some future date aluminium would be produced and sold profitably at 65 cents per pound. I was not so far out, for at this date, 1891, the Pittsburgh Reduction Company offers it for sale at $1.50 per pound, and an inferior quality at 90 cents per pound, although in 1856 I was obliged to pay $2.50 per ounce avoirdupois for the metal which I was using in my experiments. From the counter-electromotive-force apparent in the circuit, I was led to think that several crucibles or tanks arranged in series would be worked more cheaply than a single crucible or tank. My reasonings on this point led me, in 1885, to apply for a patent, and on April 7th of that year patent No. 315,266 was granted to me, which shows crucibles arranged in series in the circuit. About the year 1863, I commenced making experiments on alloys of aluminium with various other metals, such as copper, silver, zinc, tin, nickel, cadmium, iron, etc. I patented two of

these alloys, one of which No. 38,301 covered the composition of an alloy which so closely resembled 18 carat gold as to deceive the most experienced dealers in that metal when judging merely by its looks. I made of this alloy watch cases, chains, thimbles, knives, forks, spoons, and some of these are now in good condition.

The second patent, No. 44,086, was for an alloy which should be adapted for sheathing metal for ship's bottoms, but the then price of aluminium was prohibitory. I made of this alloy keys, ship's nails, balls for grinding gunpowder without striking fire, and many other useful things. I had a blacksmith draw out one of these nails to double its original length without cracking it in the least, and although it was stretched from 2¼ to 4½ inches in length, yet it showed no flaw. I drove a nail which was 1¼ inches in length, into a block of lead its whole length without seriously bending or crippling it. I presume that at that date I compounded more than one hundred of these different alloys. I look upon the alloys of aluminium with other metals as a most promising field of investigation. I tested wires made from some of these alloys which did not break short of 130,000 lbs. per square inch. The shrinkage of these alloys when cast is remarkable, and needs to be provided against. Among the many of these alloys which I compounded was one which had a peculiar "cry" when passing through the rolls, resembling block tin when it is bent, only that it was much louder, and it lasted as long as ten minutes perhaps, and it could be heard at the distance of 75 to 100 feet.

I come next to the very interesting paper of Mr. Brown. He says in effect that there is at Boonton a dynamo weighing over 19 tons which will, at an armature velocity of 220 revolutions per minute, maintain a current of 3,500 amperes under a pressure of 35 volts; of course the total resistance of the circuit must not exceed .01 ohm in order for this current to be possible. This resistance must include the resistance of the crucible, plus that of the dynamo, plus that of the leading wires or conductors which complete the circuit. The distance of the dynamo from the crucible is stated to be about 14 ft. and the cross-section of these conductors is given as about 4 square inches; hence this leading wire resistance may be about .0008 ohms. He does not give us the resistance of the crucible nor its complete dimensions nor the specific resistance of the contents of the crucibles; all we can infer is that the total resistance of the circuit cannot exceed .01

ohm. Now a current of one ampere can liberate .00114 lbs. of aluminium per hour, hence a current of 3,500 amperes can liberate not more than 3.99 lbs. of aluminium per hour, or 95.76 lbs. in a day of 24 hours, and it would require at least 164.1 h. p. to turn the dynamo while doing it, were the efficiency of the system unity, but as its efficiency probably does not exceed 90 per cent. it would require 183.75 h. p. for the liberation of the 3.99 lbs. in one hour, or at the rate of 46 h. p. for the liberation of one pound of aluminium.

In a recent lecture delivered in Boston by Mr. Alfred E. Hunt, President of the Pittsburgh Reduction Company, he states that it requires about 22 h. p. to liberate one pound of aluminium per hour at their works, and he gives the production as not far from 500 pounds daily. Possibly even this is not the best attainable result. Suppose that we should employ four crucibles in series in the circuit, as illustrated in my patent No. 315,266 issued April 7, 1885, each crucible having an internal resistance of .005 ohms. Suppose further that the dynamo and its leading wires should offer a resistance of .002 ohms, then the total resistance of the circuit would amount to .022 ohms. Now instead of employing a current of 3,500 amperes suppose that the current should be reduced to 1,000 amperes, then our hourly production of aluminium from the four crucibles would amount to 4.56 lbs. or 109.4 lbs. in a day of 24 hours. In order to maintain a current of 1,000 amperes in this circuit of .022 ohms resistance against a counter-electromotive-force of 11.28 volts, it would be needful that the dynamo should be able to produce a pressure of 33.28 volts.

Next, suppose that we put ten crucibles instead of four into the circuit, with a dynamo that has an internal resistance of .001 ohms, this includes the resistance of the conductors which lead from the dynamo to the crucibles; suppose further that each crucible with its leading wires should offer a resistance of .001 ohms, then our total resistance in circuit will be .011 ohms; suppose also that our current remains as before 1,000 amperes, then our hourly production from the ten crucibles will amount to 11.4 lbs, or to 273.6 lbs., in a day of 24 hours. To maintain this current of 1,000 amperes in this circuit of .011 ohms, will require that the dynamo shall maintain a pressure of 39.2 volts, and the power required to turn the machine will be 58.8 horse power and the hourly production of one pound will require the exertion of only 5.15 horse power, instead of 46 as in Mr. Brown's

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case, or 22 in Mr. Hunt's case. Even this economy can be surpassed if we attentively study the following equation: Let $p = .0015$ be the horse power corresponding to one volt-ampere, allowing for an efficiency of 90 per cent. in the dynamo; let $q = .00114$ be the quantity of aluminium liberated in a crucible in one hour by a current of one ampere; let N denote the number of crucibles arranged in series in the circuit; let C denote the strength of the current in amperes; let V denote the pressure in volts exerted by the dynamo; let e denote the counter electromotive force encountered in each crucible, I compute its value to be $e = 2.82$, with a possibility that it may be less at the actual temperature of reduction. Let T denote the time that the current is active in the production of W lbs. of aluminium. Then $W = q\ N\ C\ T$, also the power P exerted by the engine will be denoted thus: $P = p\ V\ C$ and the latter divided by the former will be thus expressed $P \div W = p\ V\ C \div q\ N\ C\ T$; now cancelling C from both numerator and denominator we get $P \div W = p\ V \div q\ N\ T$, and if T be unity, and if for $p \div q$ we substitute their value $.0015 \div .00114 = 1.315$ we get $P \div W = 1.315 \times V \div N$. Now substitute for V its value in terms of $C\ B\ R\ N$ and e and we get $P \div W = 1.315 \times (C\ B + C\ N\ R + N\ e) = 1.315 \times (C\ B \div N + C\ R + e)$, which shows that the power per pound required diminishes as C, B, R, diminish, and as N increases, e remaining constant all the while being fixed by the laws of chemical action. If we allow $\frac{\$.05}{22}$ as the cost of one horse power according to Mr. Hunt's estimate given at Boston, and if we allow \$.06 to be the cost of the ore required to produce one lb. of aluminium, allow also \$.01 for the cost of the chemicals used in purifying it and preparing it for the crucibles; allow \$.02 for the cost of the carbon anodes consumed per lb. of aluminium produced; allow also \$.06 for labor, superintendence, depreciation, interest, etc., substituting our estimate of possibly 5 horse power per lb. of aluminium per hour, we get its total possible future cost per lb. down to less than \$.17 per lb., or to less than \$340 per ton, and if it be sold at \$.25 per lb. or \$500 per ton, an establishment which could turn out only ten tons daily would be in the receipt of a daily profit of \$1,600 or a yearly income exceeding \$480,000, which would surely pay good interest on quite a large capital; what then would be its profits were it to charge \$1.50 per lb. as the Pittsburgh Reduction Co. charges at the present date of writing? But I must leave this interesting discussion for abler hands than mine.

Discussion.[1]

Prof. Francis B. Crocker:—Mr. President and gentlemen of the Institute: There are two points in connection with aluminium which I wish to consider particularly.

The first relates to the properties and the second to the production of aluminium. I think that Mr. Brown spoke very truly in regard to the exaggerated ideas of the advantages of aluminium, but I do not think he went quite far enough. The popular idea of aluminium is that all the good properties of all its alloys are attributed to aluminium itself. It is like one individual having all the good qualities of all his relatives attributed to him personally. This is, of course, an absurd fallacy. Nevertheless, it is one that is made in every newspaper article on the subject, and I am sorry to say it is frequently made in technical journals. The most common mistake of the kind is to attribute to aluminium itself the tensile strength of aluminium-bronze, which exceeds that of wrought-iron, and is about as high as good specimens of steel. It is really one of the most tenacious metals known, but its composition is about nine parts of copper to one part of aluminium; yet its tensile strength is almost invariably attributed to pure aluminium, having a specific gravity of 2.6, which is only one-third that of iron. They take the low specific gravity of pure aluminium and the high tensile strength of aluminium-bronze. As a matter of fact, the specific gravity of aluminium-bronze is higher than that of iron. Therefore we do not have lightness combined with high tensile strength. Turning again to aluminium itself, its tensile strength is about 26,000 pounds per square inch; in other words, about one-third that of iron. This just balances the difference in specific gravity, but still leaves the advantage in favor of iron, because one-third of the volume has the same strength and the same weight as a rod of aluminium. Now the same exaggeration is applied to its other qualifications; for example, its chemical properties. Aluminium-bronze is remarkably free from tarnishing. A bright surface of aluminium-bronze may be exposed to the atmosphere for several months and retain its polish. That property is attributed to aluminium itself. Aluminium, as Mr. Brown said, is not particularly liable to corrosion, but if it is subjected to salt water or alkaline water, or even ordinary moist air, it does tarnish and oxidize rapidly. Salt water is quite hurtful to it. Therefore, freedom from oxidation is another property that is wrongly attributed to it; and the objection to all this is that it gives a very wrong impression in regard to what would otherwise be a good and legitimate object of admiration. I am not prejudiced against the metal aluminium, and would be very glad to see it used extensively, but I do not think its extensive use will be hastened—in fact I feel quite sure it will be retarded—

1 By Messrs. Crocker, Birdsall, F. L. Pope, Stetson, Wolcott and Sheldon.

by absurd exaggerations of its good qualities, because when people do come to use it they will be greatly disappointed. This common idea, for example, that it has 100,000 pounds tensile strength per square inch, and about one-third the specific gravity of iron, would lead any one to serious difficulties. Taking the general public, ninety-nine persons out of every hundred have entirely wrong ideas respecting it. There are certain things that seem to have a glamour about them—a word which Mr. Brown uses and which expresses the fact perfectly. Storage batteries and aluminium seem to be particularly favored by the public, and it has occurred to me that a combination of aluminium and storage battery—that is an aluminium storage battery—would be simply irresistible. [Laughter.] This sensationalism is really, I think, a bad thing for a new enterprise, because it is only destined to severe disappointment, but aluminium really has excellent qualities and can live down this gross exaggeration of its good qualities.

Now, taking *seriatim* the actual properties of the metal, and considering them as mechanical, chemical and electrical, we have under the mechanical head a specific gravity of about 2.6, which is almost exactly one-third of iron, and its tensile strength is 27,800 pounds, according to Mr. Brown, which is again almost exactly one third of ordinary wrought-iron. Therefore, iron and aluminium have equal strength, weight for weight, but iron has, as I have said, the great advantage of one-third of the bulk for the same strength and weight. Furthermore, any metal which "can be easily cut with a knife" is not particularly good for structural purposes as a metal. In my opinion it is too soft altogether. It is very much such a metal as zinc or tin. In fact one can hardly tell the difference except by the weight, in filing or cutting it; and certainly tin and zinc are seldom used for structural purposes, and I do not think they would be, even if they were very much lighter than they are.

Taking up the chemical properties—I have already alluded to the ease of oxidizing, which, as I say, is not excessive, but still about equal to other metals, such as tin, zinc and iron. It is easily, in fact, eagerly attacked by acids. It is not much affected by sulphur or sulphuretted hydrogen, which is a great advantage. It is very freely dissolved by alkalies, whereas most other common metals are not. In an ordinary alkaline solution, aluminium is dissolved, as most metals are, in acids, with the evolution of hydrogen. This is quite a serious disadvantage for a common metal, which is liable, for example, to have soap used upon it. We do not expect metals to be attacked by alkalies.

As to the electrical properties, of course conductivity is about the only one. The electrical conductivity is about twice that of copper of equal weight, and is about half that of copper for equal volume. Where it is important to have a light wire, and the size is not to be considered, you can use an aluminium wire,

because you get an advantage of 2 to 1 in weight. But when you consider size; for instance, a wire which has to be covered with insulating material that is expensive, then you would have a disadvantage. The figures happen to be very simple in all these cases.

In regard to the manufacture of the metal, that, I think, is an electrical monopoly, for the simple reason that I do not think anything else can successfully compete with electricity in the production of a metal the chemical affinities of which are as high as those of aluminium. Aluminium has great chemical affinities, ranking about half way between the ordinary metals and metals like sodium and potassium. These chemical affinities exhibit themselves when it is attempted to separate aluminium from chlorine or oxygen, for example. The ordinary smelting process does not seem to have ever been successful in the reduction of aluminium. So far as I know, aluminium has never been successfully reduced as iron or other ordinary metals are reduced by merely smelting in a furnace with coal. The nearest approach to this is smelting in presence of carbon, but with a very high temperature obtained by the electric current, higher than can be obtained by smelting. Therefore, even if we employ the process of smelting we still have to employ electricity. In any electrolytic reduction of the metal, of course, we have to employ electricity. Such a process seems to be a very simple and effective method, *i. e.*, the plain electrolysis of some compound of aluminium, the electrolyte being in a fused state between two electrodes. Now the fused state is necessary instead of the ordinary aqueous solution, because if you attempt to separate aluminium in an aqueous solution you will get hydrogen instead of aluminium. In other words, water is more easily decomposable than the aluminium compound.

Thus we have the electrolytic process as a very successful process, and the fused state as apparently necessary, and not really objectionable, because the temperature of the bath may be kept up by the electrolytic current itself.

Then we have, as the only other likely substitute for these two electrical processes, the ordinary chemical method of reducing, by means of metallic sodium, some compound such as the chloride or double chloride of aluminium and sodium. But even then I think we had better make our sodium by electricity; so it would still be an electrical process. So far as I can see, and experience in the past few years seems to corroborate this view, electricity is the only commercial means of producing aluminium at the present price. In the old days, when the chemical process was used, aluminium was $12 or $15 a pound. Now it is about $1, and I doubt if any chemical process could compete on this basis. Therefore, the reduction of aluminium seeems to belong to electrical science, and it is certainly a very interesting and important application of

electricity, and is, I think, destined to be perfectly successful. The purity of the metal does a great deal, as Mr Brown says, towards improving its quality. It does make a very much better metal of it. The impure aluminium is really a poor metal in almost every respect except mere lightness. But empty space is much lighter than aluminium. So there is no particular object in paying a dollar a pound to fill up space with something that is merely light, if it has no other desirable qualities.

DR. SAMUEL SHELDON:—I was very much surprised two weeks ago in taking up a piece of aluminium that came from the Pittsburgh Reduction Company, to notice its sonorous qualities. I suspended a piece, which was about six inches long and an inch and a-half wide, and perhaps three-eighths of an inch thick, at two nodal points by means of a string, and then hit it with a lead rod. Its tenacity, the way it held on to the vibrations, and the sweetness of the tone were remarkable, and I thought aluminium might perhaps have application in the manufacture of chimes or bells where lightness would come in. It seemed to have sonorous qualities very similar to those of bell metal. I do not know whether anything has been done in this direction or not.

MR. F. L. POPE:—I have always felt a good deal of interest in aluminium as a metal, ever since the time, many years ago, when the now venerable Moses G. Farmer said to me: "There is seven dollars worth of aluminium in every brick, if we could only get it out, but," he continued, reflectively, "I suppose if we should finally get it out it would no longer be worth seven dollars." So it was not without gratification that a little over two years ago I accepted a commission in behalf of the parties who subsequently established the plant at Boonton of which Mr. Brown has told us this evening, to visit the works where the Heroult process was then being carried on, at the Falls of the Rhine in Switzerland, and make a careful investigation of it. I had thought to report here to-night some of the results of my observations at that time, but Mr. Brown has treated the subject so exhaustively from every point of view, that he has left me nothing to say other than to corroborate his own statements. I paid careful attention to the cost of production and examined the items in great detail at the time, and I think I can corroborate the statement of Mr. Brown, that aluminium can now be produced in large quantities by electrical processes at a price very nearly as low as the equivalent bulk of copper. One thing that struck me very forcibly was the small amount of manual labor required in the works, in proportion to the value of the product. They were turning out at the Rhine Falls a good many hundred pounds a day of aluminium-bronze, and employed only six men, five of whom were ordinary day-laborers. I was also particularly impressed with the admirable performance of the dynamos used in Switzerland, which are precisely the same as the one at Boonton which Mr. Brown described. There were two of them coupled

together and to a vertical turbine and rated at 150 horse-power each—a very moderate rating I should say. I saw the ammeter, on one occasion when I was watching it, indicate 27,000 amperes. The current heated the loop of the conductor which surrounded the ammeter, which was a copper bar three and a-half inches square, very hot indeed, yet very little sparking was perceptible at the brushes of the dynamo. The machine gave a sort of a grunt when this great load was thrown on, but that was about all the effect that was observable. In preparing a report of my observations in Switzerland, I was considerably surprised at the limited number of industrial uses that I could think of, or could discover, for pure aluminium, and this circumstance led me to much the same conclusion that Professor Crocker has expressed here to-night, namely: that the commercial utility of pure aluminium as a metal has been very greatly exaggerated. His remarks, however, bring to mind one of Mr. Edison's characteristic observations, to the effect "that there is nothing quite equal to the storage battery for developing one's latent capacity for lying." I think in that respect the metal aluminium has been a close second to the storage battery.

Mr. E. T. Birdsall:—There is one point that I hoped Mr. Brown or Mr. Crocker would touch on, and that is the new name for the metal. Instead of calling it aluminum or aluminium we have heard a good deal of newspaper talk lately about calling it alium. Maybe Mr Crocker or Mr. Brown can enlighten us about that.

Mr. Brown:—As far as the name aluminium is concerned, I suppose it is a very much discussed question, but, if I am not mistaken, the dictionary gives it as aluminium or aluminum—both being correct. But in regard to the pronunciation, of course there are a good many who pronounce it one way and a good many the other. I think, however, that persons who are working directly with the metal would have the tendency to pronounce it aluminium, for this reason—the oxide from which the aluminium is extracted is alumina; now, the generality of workmen are not particular about how they pronounce words, and when you speak of alumina and aluminum (instead of aluminium) there is a tendency to get the two confused. Whereas, if you call the finished metal aluminium that does away with all possible chance of confusing it with alumina.

A Visitor:—I would like to say a word or two about a difficulty which has been found in the working of aluminium, namely that of joining pieces for small structures, as is usually done in the case of other metals by means of solder. I noticed in the book which Professor Rogers has written on this subject, that he says that the difficulty of joining pieces in this way has been recognized almost ever since the metal has been known, and also that in such processes as have been used for soldering different combinations of metal,

different compositions of solder have usually been employed. Now, very lately, there has been brought to my attention a method of soldering aluminium which, I think, may have a tendency to extend its usefulness in the arts, and that is a soldering process which appears to be perfectly successful. Unlike all processes that I know of, except one that is described in the patent of Mr. Selden, it resides in the flux which can be used with almost any solder. While, at the present time, by reason of the fact that some patents are pending, I cannot tell you what the process is; nevertheless, I have some samples of pieces of aluminium which have been soldered together, which I shall be very glad to place before you and give you an opportunity of examining them. It seems to me that this is a perfectly feasible method, because I have soldered pieces of aluminium together with an ordinary gas forge and with solder of various kinds, the only prerequisite being a heat sufficient to melt the solder, and this flux, which is spread along on the joined edges of the metal, and the solder then introduced. This flux was discovered by two men who had no previous knowledge of aluminium whatever. They were ordinary workmen, and fell on it almost entirely by accident. Their attention was called to it by the fact that the mill in which they were employed took a contract to make a quantity of aluminium buttons. The people supposed they could cut that aluminium into sheets and solder on shanks with eyes on them, to the back; so they took this contract. They attempted to solder the eyes to the backs and found the solder would not stick. That set them thinking about it, and they began to try various experiments, and they finally hit upon this substance, which is a compound salt, which served perfectly, and it seems to work with any sort of solder, and the result is really very satisfactory

Mr. Brown:—Professor Crocker spoke of storage batteries as being about on a par with aluminium, and he also said that if a storage battery made out of aluminium was in existence the stock of the concern selling it would go pretty high. I received a letter the other day from a gentleman in this city who has been working on storage batteries. In conversation with him some three months ago, he had expressed to me the opinion that storage batteries could be made of aluminium In the letter alluded to he intimated that he had succeeded and that the cells were on exhibition in a certain place. I haven't had a chance to see them yet, but hope to very soon.

In regard to the sonorous properties of the metal, it certainly has a very fine tone in the shape of bars, but not when made up in the shape of bells. If the metal is in the shape of a bloom ready for rolling, or something like that, and if it be suspended by a wire, it has a very beautiful tone indeed.

Mr. Townsend Wolcott:—As I understand the subject, it is the rolled aluminium which has the good sonorous qualities; is it not?

MR. BROWN:—Cast aluminium has the same—and, if anything, better than when rolled.

MR. WOLCOTT:—I was going to suggest that if it was spun up instead of being cast it would have a better tone.

MR. BROWN:—I think not; although when made into the shape of gongs it has a fairly good tone.

MR. ALBERT STETSON:—I want to call attention to one fact which Mr. Brown did not mention in his paper. We all of us heard of for many years and probably wondered why aluminium had not come more into general use. The trouble with aluminium is not in the making of it, but in the handling of the metal after it is made. There are only very few people in all the world who know how to handle the pure metal and cast it, and even those people, with all their experience, very often get poor castings, and so their work becomes very expensive. Good work can be done by teaching the manufacturers of the country what to do with the material after they get it.

The Secretary read the following obituary of Mr. Thomas P. Conant:

OBITUARY.

THOMAS P. CONANT was born July 11th, 1860, at Paris, France. He was carefully educated, and graduated at Columbia College School of Mines, as a mining engineer in 1882. He then became Mr. Edison's Secretary, and subsequently served in the Construction Department of the Edison Company, where he continued two years. He gained further electrical experience with the United States Illuminating Company and the Bradley Electric Power Company. After an engagement of one and a-half years with the Electrical Accumulator Company, he re-entered the Edison service in August, 1889, and was appointed Engineer of the Eastern District of the Edison General Electric Company, a position which he occupied at the time of his death, which occurred on February 24th, 1891, at the home of his grandfather, the Rev. Dr. T. J. Conant, in Brooklyn. Mr. Conant was elected an Associate Member of the American Institute of Electrical Engineers, November 1st, 1887, and was transferred to full membership January 3rd, 1888. He was also a member of the American Society of Mechanical Engineers, the Electric Club, the Manhattan Athletic Club, and other associations. In his leisure moments he devoted much time to literature, and several poems from his pen have appeared in *Harper's Magazine* and other periodicals. He made several improvements in secondary batteries and their application, patents for which were assigned to the Electrical Accumulator Company.

MR. H. A. FOSTER:—I offer the following resolution:

Resolved, That the President be and is hereby authorized to appoint a local committee of five to co-operate with the Secretary in making arrangements for the general meeting in May.

[The resolution was adopted, and the Chair appointed as the Committee, Mr. H. A. Foster, Mr. Thorburn Reid, Mr. Harold Binney, Mr. Edward B. Ives and Mr. Edward Caldwell.]

Mr. N. D. C. Hodges then read the following paper on "Some Possible Modifications in the Methods of Protecting Buildings from Lightning."

A paper read at the 56th Meeting of the American Institute of Electrical Engineers, New York, April 21st, 1891.

SOME POSSIBLE MODIFICATIONS IN THE METHODS OF PROTECTING BUILDINGS FROM LIGHTNING.

BY N. D. C. HODGES.

I remember with what hesitation I ventured some months ago to explain to your fellow-member, Mr. T. C. Martin, my ideas on some possible modifications in the method of protecting buildings from lightning. But as he was kind enough to express the belief that there was something in what I had to say, I have ventured to come here this evening to explain my ideas to what must be an extremely critical audience.

To begin with, I have always belonged with those who have been sceptical as to the utility of lightning-rods as ordinarily placed on our houses. I was never able to understand, and cannot now, why, if a lightning-rod as ordinarily introduced was useful, the lightning should scent out a bad earth connection at so considerable a distance. I mean that I could understand the ordinary theory of the rod if invariably the electrical discharge followed the rod as far as its conductivity was good, only to leave it when the bad earth connection was reached.

I am aware that the advocates of the old form of rod, point to the apparent beneficial effects of the Harris system as introduced on ships. This may be due to the possibility of making an especially good earth connection, or it may be due to there having been introduced on ships about the time that rods were introduced some other modification which has had a beneficial effect. I have some suspicions on this point, and find it recorded in the London *Electrical Review* that lightning does not play as destructive a part as it did forty or fifty years ago, and that even

those ships unprovided with conductors have suffered less damage than a smaller number of ships experienced formerly. Not that modern vessels are exempt, but they seem to be struck in a manner which causes fewer fatal accidents, and in some cases even the effects of a lightning-flash have borne so little trace of their origin that they have been credited to the wilful act of some one on board.

Ordinarily a lightning-rod is regarded as a conduit or pipe for conveying electricity from a cloud to the ground. The idea is that a certain quantity of electricity has to get to the ground somehow; that if an easy channel is opened for it, the electricity will pass quietly and safely; but that if obstruction is introduced, violence and damage will result. This being the notion of what is required, a stout copper rod, a wide-branching and deep-reaching system of roots to disperse the charge as fast as the rod brings it down, and a supplement of sharp points at a good elevation to tempt the discharge into this attractive thoroughfare, are naturally guaranties of complete security.

I think Oliver J. Lodge has expressed well the difficulty that has always been present in my mind when I have read detailed descriptions of the effects of lightning. He says, in a paper published in the *Electrical Engineer* some time last June, that when, in spite of all precautions, accidents still occurred; when it was found that from the best-constructed conductors flashes were apt to spit off in a senseless manner to gun-barrels and bell-ropes, and wire fences and water-butts,—it was the custom to more or less ridicule and condemn either the proprietor or its erector, or both, and to hint that if only something different had been done,—say, for instance, if glass insulators had not been used, or if the rod had not been stapled too tightly into the wall, or if the rope had not been made of stranded wires, or if copper had been used instead of iron, or if the finials had been more sharply pointed, or if the earth plate had been more deeply buried, or if the rainfall had not been so small, or if the testing of the conductor for resistance had been more recent, or if the wall to which the rod was fixed had been kept wet,—then the damage would not have happened. Every one of these excuses has been appealed to as an explanation of a failure; but because the easiest thing to abuse has always been the buried earth connection, that has come in for the most frequent blame, and has been held responsible for every accident not otherwise explicable.

I have to say, therefore, that up to about two years ago I was simply in the dark as to what was the matter with lightning-rods. I could not accept the reasoning set forth in the report of the Lightning-Rod Conference or in any of our books. It did not seem to me that the arguments in support of the ordinary lightning-rod were logical. It seemed to me that there was something that we did not understand.

About two years ago one of the oil-tanks at Communipaw or Bayonne, just off the New Jersey Central Railroad, was struck by lightning; and, as I pass that way each day, my attention was again called to the question how we should protect our buildings from lightning; and one evening, taking up Silvanus P Thompson's little book on electricity, I think it was, to see what he had to say about lightning, I re-read the ordinary theory of the formation of the high potentials that are manifested in lightning-discharges.

This theory is simply this: that if, in the cloud, there is a certain quantity of electricity distributed on a given mass of fine mist, it will exist there at a certain potential, depending on the capacity of this finely divided matter. Now, if these mist-particles coalesce into raindrops, the theory points out that there would be a decrease in the electrical capacity, and a consequent increase in the potential of the charge. It occurred to me immediately, that, if this theory had any foundation in fact, it ought to be possible to reverse the operation on the surface of the earth; that is, to receive the lightning-discharge on some large body, which would then be broken up into fine particles of vapor, which would have a considerably greater electrical capacity, and that the potential of the discharge would thereby be materially reduced, and the effects of the lightning mitigated. This was my hypothesis to work upon, and I immediately began to look through the records to see what actually happened in the case of lightning discharges, and to see if there was any support in fact for my hypothesis.

The first book at hand was Sir William Thomson's "Papers on Electricity and Magnetism," and I found that he described in detail the case of a farmhouse in Scotland, which was struck by lightning, and in which this very dissipating effect took place; that is, the bell-wires were dissipated,—an occurrence which, as you know, is extremely common when a lightning-discharge takes place. I went on through the records, and found number-

less cases of this, the oldest being that of the dissipation of the metal covering on the wooden shield of some Greek warrior. I mention this case as of interest, as it brings out a very fortunate circumstance, that when thin metal is dissipated against wood or even against plaster, no harm results to the wood or plaster. Of course, you know that it has been somewhat discussed whether this action is a dissipation through the heating of the metal, or whether it is a cold dissipation—a breaking-up into particles, as it were—of the metal. On this point I have nothing to say.

But as I went on through the records I could not make the facts accord satisfactorily with my hypothesis. The dissipating action that I was looking for certainly took place, and is a very common accompaniment of lightning-discharges, but in spite of it, there was damage to the building. It was only after a considerable reading of the records that it gradually dawned on me that I had found no case where damage to the building occurred on the same level with the dissipated conductor.

Let me describe here in Franklin's own words a typical case of the action of a small conductor dissipated by the discharge.

Franklin, in a letter to Collinson read before the Royal Society, December 18, 1755, describing the partial destruction by lightning of a church-tower at Newbury, Mass., wrote: "Near the bell was fixed an iron hammer to strike the hours; and from the tail of the hammer a wire went down through a small gimlet-hole in the floor that the bell stood upon, and through a second floor in like manner; then horizontally under and near the plastered ceiling of that second floor till it came near a plastered wall; then down by the side of that wall to a clock, which stood about twenty feet below the bell. The wire was not bigger than a common knitting needle. The spire was split all to pieces by the lightning, and the parts flung in all directions over the square in which the church stood, so that nothing remained above the bell. The lightning passed between the hammer and the clock in the above mentioned wire, without hurting either of the floors, or having any effect upon them (except making the gimlet-holes, through which the wire passed, a little bigger), and without hurting the plastered wall, or any part of the building, so far as the aforesaid wire and the pendulum-wire of the clock extended; which latter wire was about the thickness of a goose-quill. From the end of the pendulum, down quite to the ground, the building was exceedingly rent and damaged. * * * * No

part of the aforementioned long, small wire, between the clock and the hammer, could be found, except about two inches that hung to the tail of the hammer, and about as much that was fastened to the clock; the rest being exploded, and its particles dissipated in smoke and air, as gunpowder is by common fire, and had only left a black, smutty track on the plastering, three or four inches broad, darkest in the middle, and fainter towards the edges, all along the ceiling, under which it passed, and down the wall."

I would thus formulate what seems to be true,—that a conductor which can be easily dissipated by a lightning-discharge protects the building to which it is attached between two horizontal planes, the one passing through the upper end of the dissipated conductor, and the other through the lower end; and it is this one point that I would urge upon the consideration of the Institute.

I have taken the time of the Institute to tell how I reached this conclusion; but, as must always be, I reached it by making some false digressions. So far as I know, therefore, a conductor such as I have here—a conductor made of light copper ribbon, so that 75 feet of it will weigh only a pound, and made in sections two feet long, which shall be tacked to the building from its ridge-pole to the foundation, the joints being made of low conductivity by the insertion of insulating washers—will protect the building. The conductor will be destroyed by the discharge. Its destruction can take place even against a plastered wall without injury to the wall; but no other harm will occur so far as the conductor extends in a vertical direction. There is no need of the conductor following the shortest course to the ground. There is no need of providing a good earth connection. I can see no difference between the two ends of the metallic ribbon. You do not attempt to make a good connection at the top with the dielectric, and I do not see why you should attempt to make a good connection at the bottom. In no case on record of the protecting influence of dissipatable conductors has this protecting influence depended upon there being a good earth connection. Of course, the ribbon should not be boarded over. Free gunpowder burns harmlessly enough, but it causes damage when burned in a confined space; and the dissipation of a conductor presents similar phenomena.

It would not do to run such a conductor as I suggest [illegible]re part way down the building, and then make it turn up ag[illegible] before

its final descent to the ground, as in such a case there would probably be a line of disaster from the point where the upward turn began.

Doubtless numerous improvements can be suggested, but letting this stand as the main point of what I have to say,—that a dissipatable conductor protects,—it may be of interest to consider why it protects. But here you will understand perfectly well that while I can offer certain explanations which seem fairly plausible to me, it is not in the nature of things that I should have gotten at the whole truth.

In order to destroy a building in whole or in part, it is necessary that work should be done; that is, energy is required. Just before the lightning-discharge takes place, the energy capable of doing the damage which we seek to prevent exists mainly in the column of air extending from the cloud to the earth in some form that makes it capable of appearing as what we call electricity. We will therefore call it electrical energy. What this electrical energy is, it is not necessary for us to consider; but that it exists there can be no doubt, as it manifests itself in the destruction of buildings.

The problem that we have to deal with, therefore, is the conversion of this energy into some other form, and the accomplishment of this in such a way as shall result in the least injury to property and life. When lightning-rods were first introduced, the science of energetics was entirely undeveloped; that is to say, in the middle of the last century, scientific men had not come to recognize the fact that the different forms of energy—heat, electricity, mechanical power, etc.—were convertible one into the other, and that each could produce just so much of each of the other forms, and no more. The doctrine of the conservation and correlation of energy was first clearly worked out in the early part of this century. There were, however, some facts known in regard to electricity a hundred and forty years ago, and among these were the attracting power of points for an electric spark, and the conducting power of metals. Lightning-rods were therefore introduced with the idea that the electricity existing in the lightning-discharge could be conveyed around the building which it was proposed to protect, and that the building would thus be saved.

The question as to the dissipation of the energy involved was entirely ignored, naturally; and from that time to this, in spite

of the best endeavors of those interested, lightning-rods constructed in accordance with Franklin's principle have not furnished satisfactory protection. The reason for this is apparent when it is considered that this electrical energy existing in the atmosphere before the discharge, or, more exactly, in the column of dielectric from the cloud to the earth, reaches its maximum value on the surface of the conductors that chance to be within the column of dielectric; so that the greatest display of energy will be on the surface of the very lightning-rods that were meant to protect, and damage results, as so often proves to be the case. The very existence of such a mass of metal as an old lightning-rod only tends to produce a disastrous dissipation of electrical energy upon its surface,—"to draw the lightning," as it is so commonly put.

Having cleared our minds, therefore, of any idea of conducting electricity, and keeping clearly in view the fact that in providing protection against lightning we must furnish some means by which the electrical energy may be harmlessly dissipated, it seems clear why it is that the use of sufficient energy to dissipate a pound of copper, leaves not enough to do harm to other objects around. The question naturally arises how much energy there is available. There is stored up in each cubic centimetre of the column of dielectric from the cloud to the earth, just before the lightning-discharge, an amount of electrical energy given by the expression $\frac{1}{8\pi} K E^2$, where K is the specific inductive capacity of the dielectric, and E the electromotive intensity, both in electrostatic units. This expression is given on p. 156, Vol. I., second edition, of Maxwell's "Treatise on Electricity and Magnetism." Substituting the values of K and E, and reducing, we find that the amount of energy involved amounts very nearly to one foot-pound for each cubic foot of air. This is, of course, a maximum value

When this amount of energy is reached in any cubic foot, the air breaks down, and the discharge takes place, and the amount of energy per cubic foot in the column of dielectric reaching from the cloud to the earth cannot be uniform, but must reach this maximum value along a central core, and diminish gradually from this value to nothing at a considerable distance. If we consider that the dissipation of this electrical energy takes place throughout the whole length of the column of dielectric

from the cloud to the earth, we shall see that all the energy that we have to care for in our lightning-rod is that existing in the section of the column contained between two surfaces passing through the top and foundation of our house respectively. I have said two surfaces, as doubtless they are not planes: presumably they are two equi-potential surfaces.

I am now coming to a point that I want to make clear, and that is, that according to the usually accepted theories of electrical action, this electrical energy is gradually stored up in the column of dielectric from the cloud to the earth, and that it is distributed in this column with the greatest amount per cubic foot along some central core, this amount not exceeding one foot-pound per cubic foot, and that this process can be continued until the stress is so great that the air breaks down, when what we call a discharge of lightning takes place, and the electrical energy disappears, of course only to take on some other form. You may say that the electricity travels from the cloud to the earth, or from the earth to the cloud, whichever you please; at any rate, there is an electrical action in a vertical direction, the discharge being supposed vertical. I will ask, however, whether it is not true that the energy involved travels along the equi-potential lines; that is, travels in the main horizontally. It seems to me that it shrinks in, as it were, from the considerable column or ellipsoid of dielectric upon the central core, where it manifests itself as heat and light in the electrical flash. It will, then, be clear how it is that in providing a body upon which the dissipation of energy shall take place we have to guard against something not coming from above or below, but coming from the side, and that this may be the explanation of why it is that, so far as I have been able to find, a dissipatable conductor protects the building between two essentially plane surfaces passing through its upper and lower ends.

Have we not, then, in the lightning-discharge, another illustration of the relation between light and electricity? If we suppose for a moment that in place of the central core where the electrical energy is dissipated we were to place some hot or luminous body, this body would constantly radiate energy into the surrounding space, and at any instant there would be in each cubic foot of this surrounding space a certain amount of radiant energy. Now, if this process could be brought to a standstill at any moment, would not the conditions be in some degree similar to those

just preceding the electrical discharge? There would be need of a certain force along the central core to maintain the various stresses throughout the surrounding medium; and if this central force were to be taken away, as it is taken away when the dielectric breaks down and the spark passes, the stresses could no longer be maintained, and there would be a vibratory transmission of the energy back upon the central core.

But let all this be as it may, the main point which I would urge upon your consideration is, that by giving the electrical energy something which experience shows it will readily dissipate, that is, a conductor of varying resistance and small size, we can but mitigate the effects of lightning-discharges, so long as the conservation of energy holds true. I will only repeat that I have so far found no case on record where the dissipation of such a conductor has failed to protect the building under the conditions already explained.

Discussion.[1]

Mr. Edward P. Thompson:—I have listened with a great deal of interest to the matter presented in the paper just read. The alleged facts seem to agree with our ideas of electricity of low potential. Electricity occurs in thunder-storms, and the best thing to do is to get rid of it. One way is by conducting it away sufficiently rapid by means of a conductor of very large surface capacity, as the conductivity of a metal as to static electricity, depends upon the surface and not upon the sectional area. This principle is applied in the ordinary lightning-rod. As I understand the speaker, he proposes to provide a system whereby the electrical energy is not conducted away, but converted into heat. In view of the conduction principle having so often proved a failure, and the conversion principle having succeeded every time, according to the researches of the speaker, and since his theory agrees with well-known electrical principles, I think Mr. Hodges has presented matter well worthy of the consideration of the Institute, and I, for one, can find no objection to his system as to correctness of principle. As to practical equipment, some incombustible non-conductor, such as asbestos, should be placed between the thin metallic strip and the structure to be protected or else the melted metal may set fire to the building.

Dr. Wm. E. Geyer:—It seems to me that the occurrence quoted here from Franklin tends to show that the ordinary theory of the lightning-rod is essentially true. The bell-wire, so far as it went in the occurrence here described, was a lightning-rod, and protected the building so far as that lightning-rod went.

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1 By Messrs. E. P. Thompson, Wolcott, Birdsall, Geyer and Hamblet.

It was not heavy enough to carry the current and it was for that reason dissipated, so that the dissipation was simply an accident. The mere dissipation, however, did not save that part of the building where the wire stopped and there was no good conductor; the building was without any lightning-rod and was more damaged than where it had even a small rod.

Mr. Townsend Wolcott:—Professor Lodge's theory of the Leyden jar discharge is that it is oscillatory under ordinary conditions, that is where the coatings are connected with a good conductor. Now, if they are connected with a bad conductor, such as a wet string, Professor Lodge says that the discharge may be only in one direction. That is, the energy is all dissipated in a single discharge, whereas, if the conductor is good, there is little energy dissipated in getting from one coating to another. So far, Mr. Hodges' theory would seem to agree with Prof. Lodge's, that if you can use up the energy of the electricity in destroying the conductor you will get rid of it more quickly than you would in any other way, and the lightning will have less effect outside of that. But there are some other points. Mr. Hodges says we do not attempt to make a good connection at the top of the dielectric. I do not exactly understand that. We do attempt to give it good connection with a conductor. If a cloud is charged, that is a charged conductor, and so long as the current is to come down to the earth we try to get as good connection with it as we can by putting points on the lightning-rod for instance. A better way to do that would be to have a flame or something of that sort. As an experiment in drawing electricity from the air, a flame is better than a point. But, of course, it would not work in a thunder-storm.

As to the point which Dr. Geyer just mentioned, that Mr. Hodges' experiments support the ordinary theory of the lightning-rod—I think his reasoning does, to some extent too, in regard to getting rid of the energy on the central core. Take the ordinary lightning-rod—the way it is intended to work is rather to prevent a disruptive discharge than it is to take care of one that has already occurred. We desire to equalize the difference of potential by drawing off the charge from the cloud before it gets to a dangerous limit. If we can do that we do not have any disruptive discharge at all. It is just like a brush discharge such as you get from a conductor with points on an electric machine. I think the fact is not questioned that lightning sometimes is discharged in that way, but not always. There is the trouble. I do not think that any one system of lightning-rods has proved successful. Sometimes a lightning-rod will take care of several discharges in a single storm, and that seems to be something which Mr Hodges' lightning-rod would not do; because, after it had been dissipated by one discharge, I do not think, even if it could be put up in a few moments, that anybody would care to be monkeying around a conductor when there was

lightning. Mr. Hodges having asked us to clear our minds of the idea of conducting electricity, seems to go further than most of the modern theorists on electricity. I think Mr. Hodges, even if he does not use the idea of electricity, will admit that we want to make a metallic way entirely down to the earth. The case is somewhat analogous to the Leyden jar. That is, two conductors separated by a dielectric. Now, we want to bridge over the whole space of the dielectric, whether you use the idea of conductivity or not. So I don't think it makes much difference whether you use his dissipatable lightning rod or a stout one.

Mr. Hodges:—I would like to bring the discussion back once more. In order to make the paper of some length, I gave some theory; but the fact as I have found it is this: I know how the books state that the ship "Jupiter" was saved from destruction in spite of her lightning-rod going to pieces. But take the fact without going to the books at all. What do the records show? I want to get a case where the conductor has gone to pieces, and where the ship has not been saved. I have not found such a case. Suppose the conductor is dissipated between two points [illustrating]. I found this to be true in every single case of a church tower being struck where the wire runs from the bells to the clock. The wire goes, and the church-tower is saved between those two planes. Now, that is a matter of record. The ship "Jupiter" had a chain conductor, and it was dissipated; and the books say that the ship was saved in spite of the conductor being dissipated. But that is a man's opinion What I want to urge on the Institute is simply this: That in every single case when a conductor goes to pieces everything else is saved. Why and wherefore, I do not care. I gave some theoretical reasoning which seems to me more or less correct, but it is unimportant. I want to bring the discussion back to simply this matter of fact: Can you cite a case where the conductor has gone to pieces, and there has been any destruction to the building between practically two horizontal planes passing through the upper and lower ends of that dissipated conductor? You will find that there is damage above and damage below, very likely. But as I went on through the Philosophical Transactions I found one case there of a thunder-storm passing over a village (it was a century ago, or more), and the people were dependent on the church clock for the time. In the morning they did not hear the clock strike. They went up in the church tower to see what the matter was, and found the windows smashed just above the bell; found the wires running from the bell to the clock were gone, and that was all the damage done. To show how small a conductor, when dissipated, will save a house, I will cite the case of a palace in France in the early part of the century. The interior was heavily gilded. The people were sitting around on gilded sofas resting against the walls—resting against thin gilded strips. A Fellow of the Royal Society visited the palace the day after it was

struck by lightning and looked over the ground. No one was killed. No damage was done to that palace as far as the gilding extended. That is, in the gilded rooms no damage was done except that the gilding disappeared: it was dissipated. When they got to the lower portions of the palace, where there was no gilt, things were smashed. But, as I say, I started out in this thing with an hypothesis which was to a certain extent wrong. I looked into the records to see what was recorded, not as a matter of opinion, but to find out what actually happened. And what actually happened was, so far as I was able to judge, that there was no case where a dissipated conductor failed to protect a building under these limitations which I state. Of course, above or below, damage did occur.

A MEMBER:—From a practical standpoint, would you kindly tell us where you put that conductor on a house, and would you put more than one strip on a building?

MR. HODGES:—Along the ridge-pole, down the eaves, down to the ground. I should avoid, at the lower end, making connection with any large masses of metal. The number of strips placed on a building would depend on the size.

I got a patent on this thing last year. I told a friend of mine that I was interested in the protection of buildings from lightning, and, a patent not being issued, I could not tell him much about it. The next day he met me and said, "Did you read in the *Post* the account of the lightning storm in Jersey yesterday?" I said, "No." He said, "There was one case where a house was struck by lightning in Jersey and the rod was smashed, but the house was uninjured." I noted it down as another case. A man who was in my employ some years ago came to my office. I described this thing to him, and he said, "I have been there." He said when he was a boy he had a telegraph line running from his house to a neighbor's house. It was made of piano wire, and the lightning struck the roof somewhere *there* [illustrating], then followed along the metallic gutter to a point *here*. This piano wire ran down to the ground, and ran over here to the neighbor's house. At this point a little damage was done to it. The discharge followed along the conductor without doing any material damage, and there was no other damage to the house except that the wire was gone.

MR. WOLCOTT:—Although I do not question that conductors work that way, we also find that they work the other way, according to the old theory, in very many instances. It certainly is a matter of record that conductors have sometimes carried off several discharges in the same thunder-storm, which a dissipatable conductor could not do unless you were able to put up a second one in the place immediately after the dissipation of the first.

A MEMBER:—The Washington Monument is a pretty good lightning arrester. I was shown, a few weeks ago, by Prof. Owens, where lightning had struck and knocked out big chunks

of stone from the monument. He seemed to think that lightning followed the path of least electrical resistance, so he put up an additional wire and connected that with the new iron work of the monument, and he says he has not had any trouble since then with stones being knocked out.

MR. E. P. THOMPSON:—I have not heard of any experiments being performed upon Mr. Hodges' proposed system. It may seem, perhaps, impossible to perform experiments with lightning in a laboratory, because of the inconvenience of waiting for a thunder-storm; but it can be done with the induction system, and possibly, therefore, some way may be thought of for testing Mr. Hodges' invention. About three years ago, I tried some experiments in connection with a client, a well-known lightning-rod manufacturer, Captain Hubbell, who has equipped government magazines. His new system was tried with considerable elaborateness in the Equitable Building during its repair, for the consideration of the Standard Oil Company, who met with great losses of oil tanks, caused by lightning. An immense Leyden battery was charged with an electrical friction machine, and artificial lightning was thus generated. Small oil tanks containing alcohol—more easily lighted by the spark than petroleum—were equipped, and by discharging the battery it was easily determined how many times out of a hundred the Captain's system was successful. Some experiment with Mr. Hodges' system would soon settle the question of effectiveness.

MR. CHARLES STEINMETZ:—To one point more I wish to draw attention. By using such an interrupted conductor of small cross-section, that is of comparatively high resistance, you are liable to change the whole nature of the lightning discharge. You change it from an oscillating discharge to a steady and continuous rush of current, from which you must expect quite different effects.

When, for instance, you discharge a condenser by a conductor of very low resistance, you get an oscillating discharge of an extraordinary high frequency. If you increase the resistance of the conductor the number of oscillations of the discharge decreases, it runs down quicker, until at last you reach a value of resistance where only one wave of discharge current appears, that is, the discharge of the condenser becomes steady.

Now, if we can make a lightning discharge steady, instead of oscillating, then we have first to expect that the electricity traverses the lightning-rod only once, slowly increasing in current strength and then decreasing again by going down to the ground; while in an oscillating discharge the current will rush to and fro through the conductor until its energy is consumed by the resistance of the lightning-rod, or by electro-magnetic radiation and reradiation from the induced currents produced by the oscillating discharge in neighboring conductors.

This, perhaps, may account for some of those phenomena

mentioned to-night; that, when the lightning-rod is dissipated, that is when its resistance was very high in comparison with the quantity of electricity rushing through, there was a steady discharge and no harm was done. While, when it is an oscillating discharge, the slightest irregularity will cause the discharge to "jump the track," that is to leave the lightning-rod, which is obstructed by the counter E. M. F. of self-induction, and to spark over to metal masses of larger condenser capacity.

For what I consider as the most dangerous part of lightning discharges is not the enormous voltage of the discharge, nor the strong current rushing through the lightning-rod, but the electro-magnetic field of force, which alternates with enormous frequency and reaches far out into space, from the real path or centre of disruptive discharge, and thereby must cause inductive effects everywhere, which as before said, cause not only the main discharge to spark over, but produce true secondary or induced lightning discharges.

Hence I must be very much in favor of every arrangement which is able to change the oscillating discharge into a steady rush of current.

The *resistance* of the lightning-rod I consider as of subordinate importance only, except so far as carrying capacity is concerned. For of what use is a resistance as low as a few ohms, when the self-induction of the lightning rod causes a spurious resistance of perhaps hundreds of thousands of ohms.

MR. HODGES:—I would bring this discussion back once more to this matter of fact that I am interested in. The theory I do not care so much about. It may be interesting as mental gymnastics. I came here feeling quite sure that somebody would stand up and say, "I know a church or a house in this town or that town where the conductor was dissipated and yet damage was done on the same level." I have not found a case.

DR. GEYER:—In a disruptive discharge the length of the lightning-rod it seems to me is a very small part of the total path. I should imagine that any resistance the conductor would have would be such a small part of the total that it would not have much effect on the character of the discharge.

MR. STEINMETZ:—I believe I have been misunderstood in what I meant by the influence of the resistance upon the nature of the discharge.

Indeed, the whole resistance of the lightning circuit is so large that under any circumstances the resistance of the lightning-rod is imperceptibly small.

But, as explained in my former remark, it is not the resistance proper, but the consumption of energy by the resistance, which causes the amplitude of the oscillating discharge to decrease slower or quicker until, for a very rapid consumption of energy by resistance, only one wave appears that is a steady or continuous current.

This phenomenon is similar to a pendulum oscillating in a liquid: the greater the frictional resistance of the liquid the quicker the amplitude of the pendulum motion decreases, until, at last, in a very tough liquid, the pendulum comes to rest without any oscillation at all: periodically.

In such a way the resistance of the conductor, by consuming the energy of the electric discharge, could change the discharge from an oscillating to a continuous one, although the whole "resistance" has still about the same value "infinite," if we were allowed to speak with the usual meaning of "resistance" of *disruptive* discharges, which we are not.

MR. BIRDSALL:—I think Mr. Hodges has given us the most original idea on this lightning-rod question that has been put forward for some time. I also think that Mr. Steinmetz has hit the nail on the head in his explanation of it. It only shows us again what we do not know about the various phases of alternating currents. His theory also gives me a little uneasiness, because I have advised a number of friends who have built houses in the country to put in a metal lath, as I thought that, having plenty of metal around, that, if the house happened to be struck, it would go to the ground through this metal lath. Now, if any of those houses are struck and that metal lath turns into gas I think I shall emigrate.

MR. HODGES:—That metal lath reminds me—I wrote to Edward Atkinson about this. You know he is president of about the only insurance company in the country that cares about stopping fires; that is, reducing the amount of damage done. He wrote back that they had had experience with lightning-rods, and that their experience was such that they had abolished them on all factories that were insured by the Manufacturers' Mutual Fire Insurance Company. Now, in the mills there is a considerable surface of metal; and they find, as is natural, that the discharge spreads itself probably over the surface of this metal. At any rate, the potential was so reduced as to very materially mitigate the effects. As Mr. Atkinson put it, it spreads out over the surface of the machinery, so that no great damage is done. But they have taken off their old rods.

MR. WOLCOTT:—There is one question I would like to ask in regard to that drawing on the board—if you do not say that no damage was done to the end of the building, in spite of the fact that the conductor was dissipated, why don't you have to say that no damage was done along the eaves, in spite of the fact that the conductor was not dissipated.

MR. HODGES:—That is a fair question. A dissipated conductor may run horizontally any reasonable distance, and then run down; and when it goes to pieces the thing is saved. But when the conductor is not dissipated, there are any number of cases where the building is not saved.

MR. WOLCOTT:—I can understand it, that a dissipating con-

ductor would very often save the building, but, according to the accounts that have been cited, it does not seem to make any difference how little there is of that metal. There must be some limit. When it gets down where a little bit of gold-leaf is going to save a building it looks rather improbable. If a little bit of metal being dissipated would save a building from a lightning discharge, then an ordinary lightning discharge would not be sufficient to dissipate some of these larger conductors which are dissipated.

MR. HODGES:—I do not pretend to understand anything about it. I have theorized upon it, but that is not important. It is only the fact, and the fact stands there until somebody gets up and shows a specific case where it does not work.

MR. BIRDSALL:—I do not think that Mr. Wolcott can hold that argument, because he has not any data on the comparative energies of these various discharges of which we have record. We have a record of the damage done in the dissipation of the conductor, but we have no record of the foot-pounds of energy in the discharge.

Now, the discharge that burnt up the gold-leaf on the wall might have been a great deal smaller than some of the discharges which burned up the larger conductors. Then another point has been raised about the replacing of the conductor immediately after it was dissipated. This will never be necessary, it seems to me, for it is a recorded or alleged fact, that lightning never strikes twice in the same place. They say that in naval combats the safest place is to put your head through the hole that the cannon ball has just come through, and if it did strike more than once the rods could be arranged on the principle of the multiple fuse and a new one plugged in as fast as they dissipated

MR. WOLCOTT:—Mr. Birdsall has been facetious on this point and I will try to be so, too. I have heard it stated that one reason why lightning does not strike in the same place twice is that the place is generally gone when the lightning has struck once. I certainly have read of several cases where the conductor has conducted several discharges to earth in the same storm. Now, with regard to gold-leaf discharge. That this charge was smaller, of course, may be true. But the fact that the discharge in each of these cases is just about suited to the size of the conductors would seem to show that there was some coincidence about the matter. If a dissipated conductor always stops the damage, or very nearly always, there is something more than coincidence about it. It seems to me that such an instance as that could not be more than a mere coincidence—that a discharge which was capable of doing considerable damage to the building where the conductor was not dissipated, should be all used up by dissipating a very small amount of metal is not probable.

THE PRESIDENT:—I will call the attention of the Institute to the fact that our usual time of adjournment has very long passed.

MR. HODGES:—Ships have been struck a number of times in the same storm. If you can cite specific cases against me, all right. I have found, so far as I know that a dissipatable conductor protects. Why, is another question that does not concern us. Why that gold-leaf protected we do not care. It did protect. There is no arguing against its being reasonable, that will set aside the fact. I thought over the matter, and have some theoretical considerations to show why it does protect, but those are not essential.

This is all I want to give at the present time. But I believe there is one other way of furnishing protection against lightning which has been ignored for a number of years. The facts have been staring us in the face. I think about the same time that Harris introduced his system of lightning-rods there was a modification made in the rigging of ships which has tended to mitigate the disastrous effects of lightning. The facts were well known long before Harris came into existence; but they were so thoroughly out of tune with all the science of that day, that they were simply ignored; so that, in fact, in the report of the Lightning-Rod Conference, there is only the title of one paper bearing on the subject. To find that paper I hunted through the Astor Library, and put one of their expert searchers to work there; and it was evidently considered of so little importance, that it had not been copied in any periodical. By going back further and further in the Philosophical Transactions, I found the same facts reported of a most positive character, and I think they have a bearing on this apparent immunity of ships when they are supplied with good conductors. I am inclined to think that it is not the Harris conductor that has been doing good service entirely, but it is something else. But all that I would have said this evening, if it had not been necessary to present a paper of some length, was that a dissipatable conductor protects.

MR. JAMES HAMBLET:—I understand the gentleman to say that a dissipatable conductor protects. I have in mind a very large building situated at the top of a hill, in a very exposed position. That building is constructed with a metal roof, entirely over the building, but having no lightning-rods. It has large iron pipes, six inches in diameter, to conduct water through the building down to the ground. That building has never been injured by lightning at all, but frequently trees around it on the hill have been destroyed by lightning. The lightning conductors of the building, which are these same iron pipes I have mentioned, have not been dissipated.

Adjourned.

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Adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

ANNUAL MEETING.

At the Annual Meeting of the American Institute of Electrical Engineers, held in New York City, May 19th, 1891, the following officers were elected to fill vacancies occurring under the Rules:

PRESIDENT,
ALEXANDER GRAHAM BELL.
Term Expires, 1892.

VICE-PRESIDENTS.
Terms Expire, 1893.
THOMAS D. LOCKWOOD,
CARL HERING
WILLIAM J. HAMMER.

MANAGERS.
Terms Expire, 1894.
DR. LOUIS BELL,
HERBERT LAWS WEBB,
PROF. ALFRED G. COMPTON,
JAMES HAMBLET.

TREASURER,
GEORGE M. PHELPS.

SECRETARY,
RALPH W. POPE.

The following reports were read by the Secretary:

REPORT OF COUNCIL FOR THE YEAR ENDING MAY 19th, 1891.

Your Council takes pleasure in reporting that the work of the Institute has been continued during the past year with most gratifying results, in the growth of membership as well as interest on the part of all who are identified with it.

Ten monthly meetings of Council have been held at which the average attendance has been 9.

Ten meetings of the Institute have been held besides the annual meeting, at which twenty-five papers and reports were read and discussed. These have been printed and distributed to the membership.

Committees of the Institute have reported upon a Standard Resistance of Copper and the adoption of American names for

electrical units. The reports of these Committees have been widely circulated and have met with general approval.

A Standing Committee of five, on Units and Standards has also been appointed.

The total membership at the close of last year's report was as follows:

Honorary	2
Members	145
Associate Members	280
Total	427

There have been 124 Associate Members elected during the past year.

The following members have resigned during the year:

G. W. Hebard, C. L. Healy,
E. H. Johnson, R. W. Blackwell,
Leopold Weil, S. M Plush,
Hartley Gisborne. Total, 7.

We have lost by death the following members:

William B. Knight, of Kansas City, Mo.
Thomas P. Conant, of New York City. Total, 2

One member has been dropped from the roll.

The total membership is now as follows:

Honorary	3
Members	161
Associate Members	377
Total	541

SECRETARY'S BALANCE SHEET

for the Year Ending May 19th, 1891.

Ralph W. Pope, Secretary, in account with the American Institute of Electrical Engineers.

Dr.		*Cr*	
1891.			
To Balance from 1890	$ 2 23	By Cash to Treasurer	$5,717 45
Sundry receipts	12 23		
Entrance fees	565 00		
Life Members	200 00		
Back dues	85 35		
Current dues	3,921 14		
Advance dues	203 33		
Electrotypes sold	222 35		
Typewriting and Stenography	217 53		
Transactions sold	270 00		
For Binding	28 80	Secretary's Balance on hand	10 51
	$5,727 96		$5,727 96

Expenses of the Year.

The Treasurer has disbursed upon warrants drawn by the Secretary, by direction of the Council and Finance Committee, the amount of $5,213.77, which has been expended as follows:

Stenography and Typewriting	$ 330 37
Stationery and Miscellaneous Printing	400 17
Postage	311 14
Messenger Service	34 99
Salaries	1,475 00
Engraving and Electrotyping	454 33
Meeting Expenses	88 00
Rent	773 00
Printing Transactions	1,112 47
Express	2 90
Moving	17 00
Telegrams	3 35
Gas	10 49
Binding	87 72
Office Fittings	70 45
" Expenses	32 79
Copyright	7 00
Duties	75
Cartage	1 85
	$5,213 77

This amount should be credited with the sum of $738.68 appearing in the receipts; for cuts, copying, sales of transactions, etc.

The net receipts for the year for membership fees and dues are therefore $4,974.82 and the net expenses $4,475.09 leaving an excess of receipts over expenses of $499.73. The outstanding bills against the Institute amount to $323.65, and the uncollected bills due the Institute from others than members amount to $334.67. The arrearages of dues for the past year amount to $480. The balance in the Treasurer's hands to the credit of the General Fund is $752.94, showing an increase of $503.68 over last year.

On the first of June, the Institute entered upon the arrangement referred to in last year's report, by which the meetings of the Institute and of the Council have been held in the House of the American Society of Mechanical Engineers, where the Secretary's office is also located. The quarters have proved entirely satisfactory, and in accordance with the terms of the lease the same arrangements will be continued for the coming year.

Respectfully submitted by direction of Council.

Ralph W. Pope,
Secretary.

TREASURER'S REPORT.

New York, May 15th, 1891.

GEO. M. PHELPS, TREASURER, in account with

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Dr.

Balance in Treasurer's hands May 14th, 1890.....	$ 249 26	
Received from Secretary, May 15, 1890, to May 15, 1891.	5,717 45	$5,966 71

Cr.

Payments from May 16, 1890, to May 15, 1891, on Warrants from Secretary, Nos. 197 to 266 inclusive	$5,213 77	
Balance to New Account.....	752 94	$5,966 71
Balance on hand May 15, 1891.....		$752 94

BUILDING FUND.

Balance on hand May 15, 1891.....	$350 00

GEO. M. PHELPS,
Treasurer.

May 15, 1891.

Messrs. Thomas D. Lockwood, Edward P. Thompson, and James Hamblet were appointed an Auditing Committee to examine the accounts of the Treasurer, and subsequently reported that they were found correct.

It was voted that $500 from the surplus, together with the $350 constituting the building fund be invested, and Treasurer George M. Phelps and Secretary Ralph W. Pope were appointed Trustees to take charge of the matter.

GENERAL MEETING.

New York, Wednesday, May 20th, 1891.

Morning Session.

The Chairman:—[Past-President W. A. Anthony.] The Secretary has one or two announcements.

The Secretary:—At the Council meeting yesterday afternoon the following associate members were elected:

Name.	Address.	Endorsed by.
Alden, James S.	Assistant Manager, with L. H. Alden, 486 River Drive, Passaic, N. J.	Henry Morton. Wm. E. Geyer. Jas. E. Denton.
Ayer, James I.	General Manager, Municipal Electric L. & P. Co., 322 Pine St., St. Louis, Mo.	Dr. Louis Bell. M. D. Law. W. J. Johnston.
Bowman, Fred A.	Electrical Engineer, N.Y. Isolated Dept., Thomson-Houston Electric Co., 65 Columbia Heights, Brooklyn, N. Y.	F. C. Bates. Jas. B. Cahoon. J. P. B. Fiske.
Colville, Frank C.	Electrician and Inventor, 1503 Seventh Ave., Oakland, Cal.	Geo F. Curtiss. R. W. Pope. T. C. Martin.
Essick, Samuel V.	Electrician, The Essick Printing Tel. Co., Yonkers, N. Y., 171 Broadway, New York City.	Chas. Cuttriss. Jos. Wetzler. T. C. Martin.
Everest, Augustine R.	Electrical Expert, Thomson Electric Welding Co., Astor House, New York City.	M. C. Sullivan. T. C. Martin. R. W. Pope.
Hancock, L. M.	Supt. of Construction, Western Electric Co., 22 Thames St., New York City.	John E. Davies. Enos M. Barton. Chas. A. Brown.
Hewlett, Edward M.	Electrical Engineer, Isolated Dept. Thomson-Houston Electric Co., 65 Columbia Heights, Brooklyn, N. Y.	F. C. Bates. Jas. B. Cahoon. J. P. B. Fiske.
Mason, James H.	Electrician and Supt., Mason Battery and Electrical Co., 120 Park Ave., Brooklyn, N. Y.	T. C. Martin. Jos Wetzler. C. E. Stump.

SMITH, OBERLIN,	President and Mechanical Engineer, Ferracute Machine Co., Lochwold, Bridgeton, N. J.	Elihu Thomson. Henry Morton. R. W. Pope.
SQUIER, GEORGE O	Lieut. U. S. A., Student of Physics, Johns Hopkins University, Baltimore, Md., Box 243.	Louis Duncan. W. F. C. Hasson. Wm. J. A. Bliss.
STONE, CHARLES A.	Manager with E. S. Webster, Mass. Electrical Engineering Co., 4 P. O. Square, Boston, Mass.	Chas R. Cross. W. A. Anthony. E. S. Webster. Francis R. Hart.
UEHLING, T. A.	Ass't Supt. and Chemist, U. S. Aluminium Metal Co., Boonton, N. J.	A. S. Brown. R. W. Pope. R. A. Fessenden.
WASON, CHAS. W.	Electrical Engineer, East Cleveland R. R. Co., 1762 Euclid Ave., Cleveland, O.	F. B. Rae. E. P. Roberts. R. W. Pope.

Those who have not already registered will find the register in the parlor, and the members and associates of the Institute are entitled to the Institute badge which has been prepared for the occasion. The inquiry has been made as to the metal of which the badges are made—whether it was alium, aluminium or aluminum. I notice that the producers say it is aluminium, and that it is over 99 per cent. in purity. An alloy of this might have been used which would have made a whiter metal, but it was thought best to have it as pure as possible in order to show exactly what the metal is.

PROF. ANTHONY:—I am very happy at this time to resign the position of presiding officer to Vice-President Lockwood, and I will ask him to take the chair.

MR. LOCKWOOD on taking the chair said:—Gentlemen of the American Institute of Electrical Engineers: It is my first duty, as well as my first privilege, to request the Institute to join with me in expressing the appreciation and acknowledgements which I am sure we all feel when we consider the dignified, impartial and urbane manner in which our late President has presided over our deliberations during the year which has just concluded. As a member of the Council and Institute I can only say that I have been profoundly impressed with the ability with which he has conducted our deliberations, and with the fairness which has characterized his every action and word. I am sure that there will be no dissenting voice when I propose a very hearty and cordial vote of thanks to Professor Anthony, our retiring president.

MR. HAMMER:—I take pleasure in seconding the motion.

THE CHAIRMAN:—It is moved and seconded that a cordial vote

of thanks be presented to Professor Anthony for his labors in behalf of the Institute for the past year.

THE SECRETARY:—I wish to add that the record of the President who has just passed out of office, in attendance to duty has been equal to, and possibly surpasses, all previous records. He has missed no meeting of the Institute during his term of office. [Applause.]

THE CHAIRMAN:—Those who are in favor of the vote of thanks as proposed and seconded, will please say aye—contrary, no. It is unanimously carried. The Secretary will spread the vote upon the record.

Gentlemen, in presiding over the proceedings of the Institute to-day, I have cause to realize that, while I am not afraid to fill the shoes of most men, I am quite unequal to filling the shoes of the President you have elected. I may, perhaps, be able to rattle around in them in a way to satisfy the ordinary observer.

I can, I think, congratulate the Institute in having selected as its President for the year to come, a gentleman who will do honor to the Institute and who, in turn, feels himself very much honored, and who is gratified in being selected as your President. I have great hopes that very frequently he will be here himself to fill this Chair, and I am sure he will fill it acceptably to every one of us.

I desire further to say that it is a position of very great honor indeed. The time has been in the past when it was a position of more responsibility and labor than of apparent honor, and yet it seems to me that those gentlemen who readily came to the front in times when we were weak, when, upon one occasion as I understand, there were but four members at the annual meeting; when, upon other occasions, it was hard to get a quorum even at the collation which came between the morning and afternoon sessions, I think all honor—although the position does not seem to involve any—is due to those gentlemen. But at the present time it is not only a national but an international honor to be elected as President of this Institute, for the Institute, while comparatively small in numbers—not having yet attained its six hundredth member—is not insignificant, when we consider the qualifications of the members, the ability of the papers which have been presented in the past year and the equal and corresponding ability of the discussions which have been had upon them. And so I have no hesitation in returning thanks for

Professor Bell for the honor which has been conferred upon him [Applause.]

By reference to the programme for the morning session it will be found that the paper to be delivered first is "On the Development of the Dynamo," by Prof. Harris J. Ryan, of Cornell University. I am informed by the Secretary that Prof. Ryan has not been able to fully prepare his paper. Consequently the first upon the list will now be read by Prof. Francis B. Crocker upon "The Perfection of Stationary Electric Motors."

PROF. CROCKER:—I place before you a sample of a small motor upon which I propose to base this discussion, as an object lesson, not only to the audience but to myself.

Prof. Crocker then read the following paper:

A paper read at the General Meeting of the American Institute of Electrical Engineers, New York, May 20, 1891. Vice-President Lockwood in the Chair.

THE PERFECTION OF STATIONARY ELECTRIC MOTORS.

BY FRANCIS B. CROCKER.

INTRODUCTION.

A STUDY of the history of the electric motor shows us that from the very beginning to the present time there has been a great tendency toward complicated, fanciful and unpractical designs, including outlandish forms of field magnets and unnecessary multiplicity of field cores and coils. In most cases also the mechanical construction has been very bad, the shaft being too small, the bearings being weakly supported and the general finish and workmanship being rough and not up to the standard of first-class machinery. From the electrical standpoint the insulation has been very poor, and shares equally with imperfect mechanical construction, the responsibility for the numerous troubles and failures which have occurred with electric motors. The magnetic circuit of motors has also been designed in a way that indicates entire ignorance of the laws governing the production of a strong magnetic field. In all these respects the electric motor has been worse than the contemporaneous dynamo-electric machine, the reason being, I think, that the men who have studied and designed the dynamo have been more competent than those who have turned their attention to the electric motor, electric power being comparatively a more recent development of electricity than electric lighting. There have been, of course, notable exceptions to the general badness of motor design and construction, but in the case of all of these exceptions, which are few, I think it will be found that the superiority of the motor in each particular case was directly due to the presence of an electrical engineer of recognized ability.

Electric motors are used for three different classes of work. First, transmission of power over considerable distances, for example from Niagara Falls to Buffalo; second, distribution of power, for example in a district of a city from a central station; and third, electric railway work.

The first of these classes of work usually requires large motors, over 50 or 100 horse-power; the second requires comparatively small motors, ranging between ⅛ and 10 horse-power; the third requires motors from 20 to 100 horse-power and a peculiar construction adapted to their special purpose. I purpose to consider in this paper, motors of the second class for distribution of power especially.

It has long been my opinion that the proper way to design a good electric motor is to consider very carefully and systematically each part, first by itself, and then with reference to the other parts, and thus collect and build up the elements which are best not only in themselves, but also in combination with each other, so that finally by putting and fitting these parts together with the best possible workmanship, the best result will certainly be obtained. The correctness of this procedure is so self-evident that it seems hardly necessary to uphold it, but every one familiar with electric designing of all kinds will admit that the usual process has been to invent some peculiar form of armature or field, and then attempt to fit the rest of the machine to this particular part, producing all sorts of anomalous and awkward constructions. It will also be admitted that experience has taught us that the simpler and less extraordinary forms of machine have usually been more successful. In fact, I believe there is no kind of electrical apparatus in which sensationalism in design has been successful, except perhaps in arc light dynamos. Let us now take up the various parts of the electric motor and consider each as a simple problem in designing.

Base.

This should be a solid iron casting, flat and simple in form and resting directly and squarely upon the base frame or floor, which latter should preferably be of wood to secure insulation. The base should be heavy to bring the centre of gravity low; in fact, this is the only part of the machine where weight is not only not objectionable, but very beneficial, and since it is merely of plain cast iron the extra cost is insignificant. A heavy base does

more than anything else to give great stability and strength to the whole machine, and is highly desirable in a stationary motor. The bearings should be directly and rigidly mounted upon this base, a simple fact that is often disregarded.

Field Magnets.

The field magnet should be bipolar except in the larger machines, above 10 horse-power, on account of the great complication caused by the increased number of armature sections, commutator bars, field coils, etc., necessary in multipolar machines, and the narrowness of the neutral or non-sparking space on their commutators; for example, multiple brushes or cross-connected armatures are particularly objectionable in small machines. The field should also be a single magnetic circuit rather than the consequent pole type, because the former is more economical in wire and current required. For example, one turn of wire around a 7 in. core is equivalent to the two turns of wire around two 5 in. cores, the cross-section of one 7 in. core being nearly equal to that of two 5 in. cores, whereas the length of wire is only 70 per cent. as much with the single core. In other words, a single circuit requires only 70 per cent. of the weight of wire and 70 per cent. of the energy of magnetization that is required by a double magnetic circuit. A triple magnetic circuit or quadruple magnetic circuit—that is, three or four cores magnetically in mutiple arc, are still more objectionable, requiring about twice as much wire as a single core of the same total cross section.

The field magnet may be arranged with respect to the base described above in three general ways: First, the pole pieces may be set upon the base (Edison type); this form requires non-magnetic material, usually a zinc plate, to be interposed to prevent magnetic short-circuiting through the iron of the base; even with a zinc plate Hopkinson found 10.3 per cent. magnetic leakage through the base of the Edison-Hopkinson dynamo.

Second, the iron base may form one of the pole pieces (Manchester type). This is also objectionable on account of the greatly increased magnetic leakage, which is practically proportional to the surface exposed to the air, and the extension of the base necessary for the bearings, etc., greatly increases this surface and also allows the magnetism to leak up into the shaft, pulley, etc., causing various troubles. This form has the further objection that it is of the consequent pole type, the disadvantage of which, I have just pointed out.

Third, the iron base may form the yoke of the field magnet with the cores and pole pieces extending upward (Kapp or "inverted horseshoe" type). This plan has neither the leakage nor other faults of the other forms and possesses the great advantage that the pole pieces are removed as far as possible from the base, bearings, etc., thereby avoiding magnetic leakage; and the base being purposely made heavy for stability and strength, as already stated, is therefore exactly suited to being used as the yoke, thus performing two functions to the best advantage. There is, moreover, no objection to using a cast iron base as the yoke to complete the magnetic circuit, since the heavy base I have insisted upon is of large cross-section and therefore of ample magnetic permeance and the increased size of the iron yoke does not aggravate the magnetic leakage because it is magnetically neutral. The field cores should be of wrought-iron because the field wire surrounds them and the use of cast-iron, which is of low permeability, would require a core of almost twice the cross-section and therefore a much greater length of wire to obtain the necessary magnetizing force. With the smaller wrought cores the magnetic leakage would also be less. The pole pieces should also be wrought-iron to reduce their size and therefor their magnetic leakage, they being at the highest magnetic potential of any part of the machine. In regard to magnetic leakage, I would call attention to the fact that it is objectionable not only on account of wasting lines of force, but because it affects electrical instruments, watches, etc., in a very troublesome manner.

The field magnet cores and pole pieces being preferably of wrought-iron, the question of uniting them to the base and to each other next arises. Since any considerable fitting or working of wrought-iron is difficult and costly, the best solution of this problem certainly seems to be to forge (*i. e.*, drop-forge) the core and pole piece in one piece. This also permits the bore or space for the armature to be perfectly formed at the same time. In connecting these cores to the cast-iron base, it is practically essential to set them into it in order to get sufficient contact and strength; since the ordinary butt joint between wrought and cast-iron, limits the magnetic conductivity almost as much as if cast-iron were used throughout. This method of putting the magnetic circuit together reduces the work of fitting wrought-iron to a minimum, which work is the only objection to the use of wrought-iron. The supposition that cast-iron is cheaper than

wrought for magnets is a fallacy, as its permeability is as much below that of wrought-iron as is its price, both being about one-half.

Field Coils.

These should of course be perfectly insulated, a result which can be accomplished with certainty by winding them on a solid spool of material which is fire and water-proof and highly insulating. These spools should never be made of material that softens by accidental heating of the wire, as this would allow the wire to cut through and make a "ground" on the core, which is just what the spool should prevent. I have tested spools of this kind which cut like cheese when a wire heated by a current was applied to them, this being a very effective method of testing this point.

Armature.

In regard to the choice between the ring and drum forms of armature, the advantage of the drum is, that it is easy to support mechanically, but it is difficult to wind, as the wire is apt to slip out of place and heap out on the ends of the drum, and more serious than anything else is the fact that the wires of a great difference of potential cross or approach very near each other, causing great liability to short circuit. The ring armature is somewhat hard to support mechanically, but the winding does not tend to slip out of place or pile out on the ends, and wires of the greatest difference of potential come at the greatest distance apart—that is, on opposite sides of the armature. This is of the utmost importance in the case of high voltage machines. In the case of large ring armatures or those with few turns of wire there is little trouble in winding, but in the case of the small ring armatures or where a great number of turns of wire are used, the ring should be split in halves to facilitate winding, and put together afterward when wound.

Toothed Armature.

The armature should have teeth or projections upon it in order to secure the following advantages:

First, to reduce the reluctance of the magnetic circuit and the weight of wire and energy of current required to magnetize it.

Second, to hold the wires in place and prevent them slipping tangentially on the core, the tendency to which is much greater than is usually supposed, there being a tangential pull of about

160 lbs. on the wires of a 10 horse-power armature 8 in. in diameter running at 1,000 R. P. M.

Third, the teeth protect the wires from mechanical injury, since any blow or pressure which the armature receives comes upon them, instead of upon the armature wire which is very easily injured and the machine thereby ruined.

TRAPEZOIDAL TOOTH ARMATURE.

A certain form of teeth I have found to possess decided advantages over the ordinary parallel-sided teeth and slots heretofore used. This form of teeth is considerably wider at the top than at the bottom which, of course, makes between the teeth a slot which is wider at the bottom than at the top. This shape of tooth and slot has the following additional advantages over those

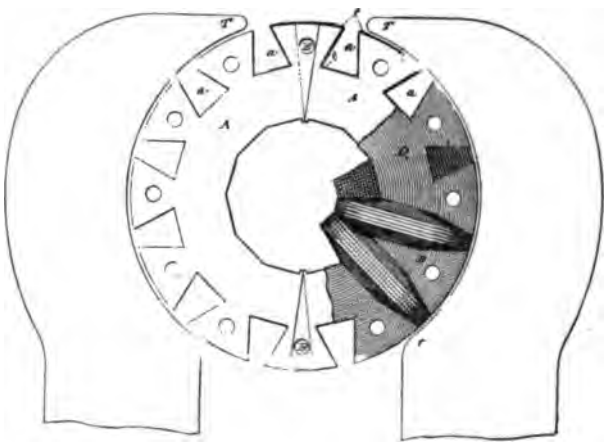

FIG. 1.—ARMATURE OF CROCKER-WHEELER MOTOR.

of the ordinary toothed armature: First, the wire is more completely held in place, being held against centrifugal force as well as against tangential force; second, the wire is also more perfectly protected mechanically, since it is almost entirely surrounded by iron; and third, the armature presents an almost unbroken magnetic surface to the pole pieces, thereby reducing to a minimum the reluctance of the air-spaces in the magnetic circuit and the magnetic leakage, and avoiding Foucault currents and loss of energy in the pole pieces. This trapezoidal form of slots and teeth can be still further taken advantage of by giving the former a shape which exactly agrees with the form of a section of winding on the inside of the ring, as shown in Fig. 1, the

interior spaces for winding being divided into the necessary number of sections by radial lines. This exact conformity between the inside and outside of the winding makes a perfectly symmetrical coil with no tendency for a single turn of the wires to cross at any point, and the coils on the inside occupy the whole space with the greatest possible advantage, whereas ordinarily the interior of the ring is liable to be overcrowded with wires. The trapezoidal form of slot is particularly economical as regards winding, for the reason that there are a greater number of turns in the inner layers which are shorter, and fewer turns in the outer layers which are longer, as shown in Fig. 1. The armature teeth make it easy to support the ring perfectly, since the bolts for carrying it may pass through the teeth without interfering with the winding. These bolts being held at the two ends by brass spiders or supports, make a very strong and convenient means of mounting the ring upon the shaft, and thereby overcome the objection to the ring armature. In the trapezoidal form of teeth which I have described there is even more space than in the ordinary form of teeth for the bolt holes, and in this, larger bolts and stronger support can be obtained. The perfect insulation of the armature core is of the utmost importance. The best material and the most complete covering should be used and the greatest possible care should be exercised to avoid short-circuits, "grounds," or broken wires.

Efficiency.

This should, of course, be as high as possible and is made a maximum by reducing all elements of loss as far as possible These losses are: Current to magnetize field, loss due to armature resistance, friction resistance of air, Foucault currents and hysteresis. It has always seemed to me that the efficiency is necessarily high if the machine is properly constructed, and if the efficiency is low it must be due to some loss, which is more objectionable in itself than because it involves a reduction in efficiency because this loss must necessarily manifest itself in the form of heat in some part of the machine; for example, the field coils, armature or bearings. This heat is liable to do more harm by its direct effect upon the heated parts, causing a burn-out or charring of insulation in the winding or cutting of the bearings, and any effect of this kind is much more serious than the inefficiency from which it results. The same fact can be expressed in an-

other way, by stating that if a machine can be run for one or two hours at full load without any part becoming overheated, the efficiency of that machine must be high unless the machine is very large and heavy for its power, in which case the surface for radiation and conduction would be great. This fact is more absolute than any efficiency test can possibly be, and very small losses which an efficiency test might not disclose would be indicated by local heating if, for example, the effects are allowed to accumulate for a half-hour of steady running.

The Motor as a Whole.

I can best illustrate the foregoing statements and recommenda-

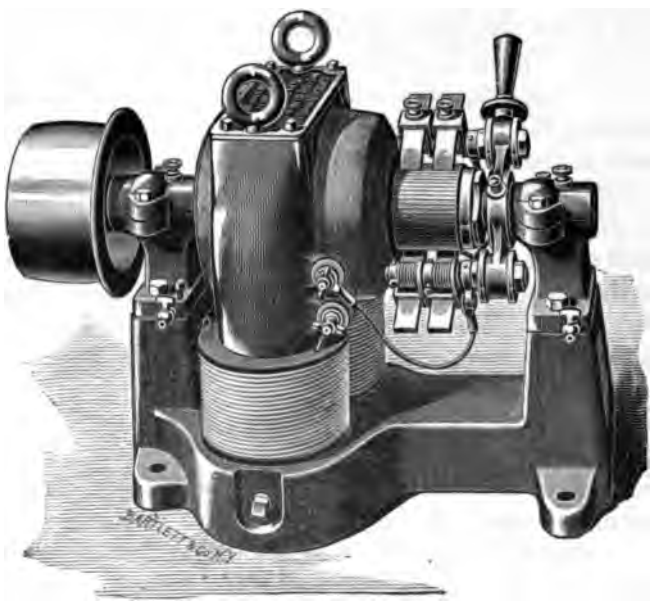

Fig. 2.—Crocker Wheeler Motor.

tions by describing the set of electric motors designed by Dr. S. S. Wheeler and myself. They were developed by precisely the same process of reasoning as that given in this paper. Fig. 2 shows the general view of the motor; each of the parts and the putting together of the whole follows exactly the statements I have already made, and it is therefore only necessary to call attention to a few points.

The fields are put together according to a method illustrated in Fig. 3, which consists in drop-forging the core and pole piece in one piece and exactly to shape; then mounting it upon a

holder in such a way that the ends of the cores are trimmed with exact reference to the armature bore, and holes being bored in the cast-iron base the proper distance apart, the cores are set into it to the depth they are trimmed off and produce a space for th armature exactly suited to it. This is much simpler than fitting the field magnets and base together and then boring out the armature space, the operation of boring being a slow one and requiring heavy machinery. The field cores set into the base are shown in the skeleton drawing, Figs. 4 and 4*a*.

The magnetic circuit produced by this method is excellent. In the first place, there are only two joints and these are of very

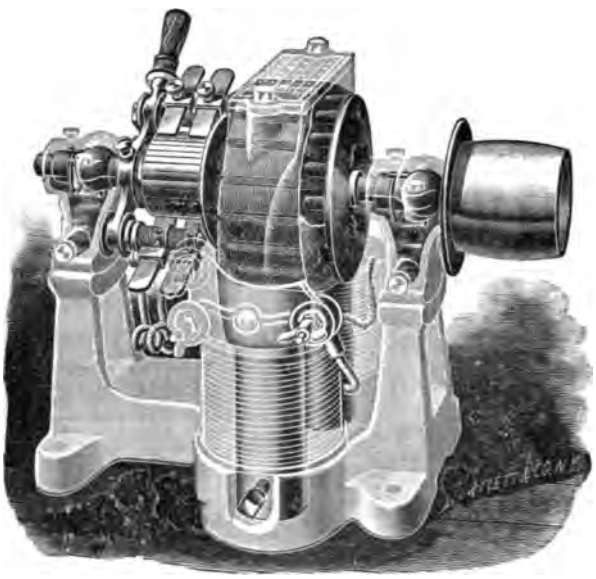

FIG. 2*a*.—CROCKER-WHEELER MOTOR.

large surface and fit very perfectly, being where the cores fit into the base. The surface of contact of each of these joints is about four times the cross section of the core and therefore about twice the cross-section of the equivalent cast-iron. The core and pole piece being made integral, avoids any joint between them as well as any necessity for expensive work in fitting them together. In fact, the only machine work required in fitting the fields to the base is simply trimming off superficially the ends of the cores and reaming the two holes in the cast-iron base, these holes being cored out in the casting.

The armature is made with trapezoidal teeth, as shown in Fig. 1, and having an almost unbroken surface of large extent exposed to the pole piece with a clearance of only one-sixteenth of an inch, the magnetic reluctance of the air gap is reduced to the lowest possible value. This exceedingly low reluctance of the magnetic circuit reduces the amount of wire required to excite the field to such an extent that only very short field coils and cores are needed, being only about one-third or one quarter of the length required with armatures having no teeth and the ordinary air

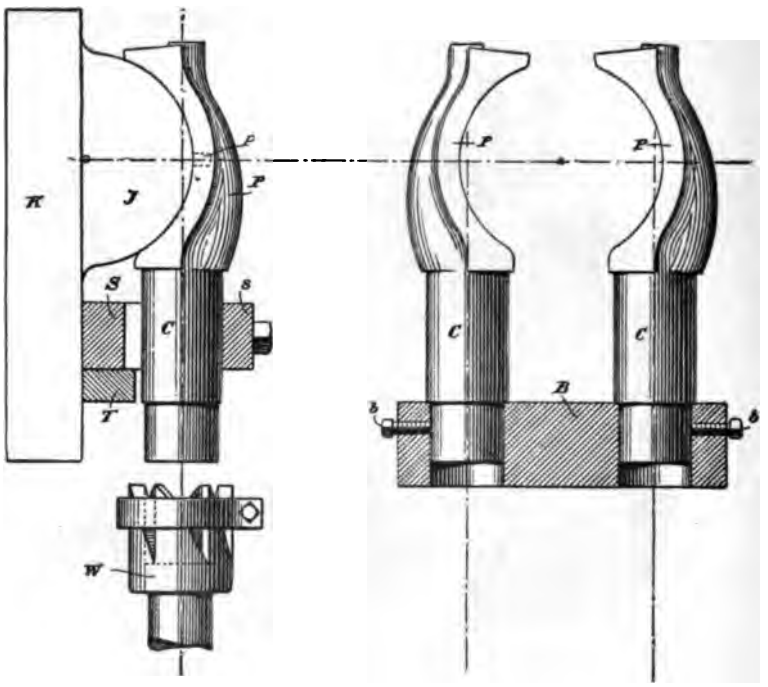

FIG. 3.

gaps in the magnetic circuit. This small quantity of wire on the field amounts to only twelve pounds in a 3 horse-power machine and sixteen pounds in a 5 horse-power machine. It has not only the advantage in economy of wire, but also makes the machine very efficient as regards current consumed in the field, being less than two per cent. in the two machines referred to. The reduction of field wire and length of coil required, is particularly important in bringing the shaft of the machine down sufficiently near the floor to give the machine stability and steadiness in run-

ning. In fact, the only objection to the "inverted horseshoe" type of field is that heretofore it has involved the use of long field coils which brought the shaft almost twice as high above the base as it is desirable to have it, giving a topheavy and unsteady result. In the type of motor described, the shaft centre is less than a foot from the floor in the 3 horse-power size, and is just about the proper height to make the pulley, belt, gear wheels or other mechanical connections clear the floor and have sufficient room to work properly. This complete overcoming of the heretofore serious objection to the "inverted horseshoe" type makes a thoroughly satisfactory field construction without any of the disadvantages of all the other forms.

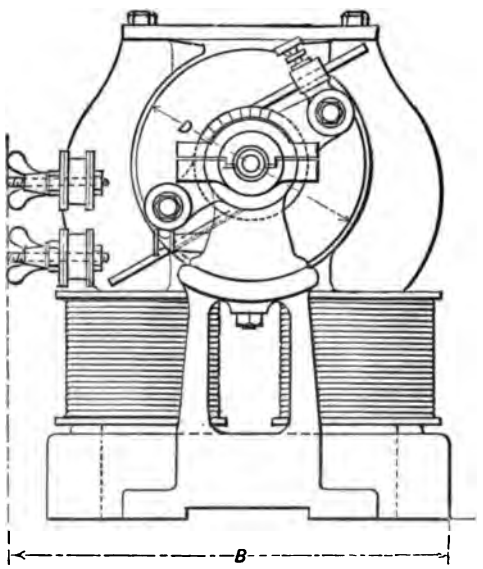

FIG. 4.

The actual results obtained from these motors, which of course is the most important and conclusive thing to know, are given in the following table and include the various mechanical, electrical and magnetic constants of the machine. All the sizes of this type of motor are made almost exactly the same in design, proportions and construction and differ only in scale, hence a description of one machine applies to all, with proper allowance for the difference in size.

Data of 3 horse-power Crocker-Wheeler motor, designed for 115 volt constant potential circuit:

Total weight	350	lbs.
Weight of field wire (No. 24, A. W. G.)	12	lbs.
Weight of armature wire (No. 13, A. W. G.)	11	lbs.
Resistance of field	310	ohms
Resistance of armature	.4	ohms
E. M. F. of circuit	115.	volts
Total current at full load	24.3	amps.
Current used to excite field	.87	amp.
Percentage of current used in field	.015	
Total current required to run free	1.9	amps.
Percentage of total energy to run free	.08	
Commercial efficiency	.80	
Armature speed	975.	r.p.m.
Total magnetic leakage or waste lines of force	.17	
Magnetic efficiency $= \frac{\text{Lines through armature}}{\text{Total lines of force}}$	83	

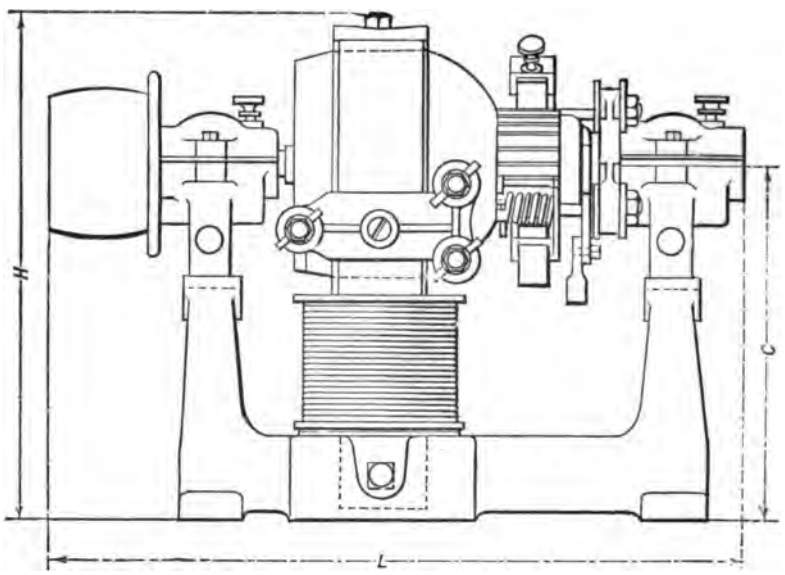

FIG. 4*a*.

The above data for the most part explain themselves, but I desire to call attention to several particularly interesting points.

The weight of field wire is extremely small, being only 12 lbs., compared with about 40 to 50 lbs. in most machines of this size. I actually found the weight of field wire to be 60 lbs. in a type of motor of equal capacity, which is claimed to have the best possible magnetic circuit.

The table also shows the current used in the field to be only

1½ per cent., which is remarkably low for machines of this size, which usually require about 5 per cent.

The current required to run free, at full speed, is also exceptionally small, being only 8 per cent.

The commercial efficiency is high for a motor of this size, particularly when the very low speed is considered, because an increased number of turns of wire on the armature is required to make up for the reduced speed, and this augments the armature resistance and causes loss. The efficiency does not vary more than about 1 per cent. from half to full load, which is quite an interesting fact.

The speed, it will be noticed, is very low for this size of machine.

The magnetic leakage or loss of lines of force is extraordinarily small, amounting to only 17 per cent. and giving what may be properly called a magnetic efficiency of 83 per cent.

This small magnetic leakage is due to the facts already stated in this paper in regard to the design of the field magnets. In this connection it should be remembered that Hopkinson in his classical investigations of the magnetic circuit found that there was 24.24 per cent. magnetic leakage in the Edison-Hopkinson machine I may also state that Mr. A. S. Ives, student in Electrical Engineering at Columbia College, found 40 per cent. leakage in a well-known dynamo (400 light) of a recent though not the very latest type.

In conclusion, I would state that it is obviously impossible in one paper of reasonable length, to completely treat the subject of motor design, but I have attempted to bring out the most important points and lay before the Institute the best results of considerable study and experimental work on the part of Dr. Wheeler and myself in the direction of improving and perfecting the design and construction of stationary electric motors.

There is one single merit in the paper to which I lay claim, and that is the fact that the ideas I have advanced are embodied in a large number of actual working machines which are open to any one for examination and criticism, whereas the design of electric motors is usually given on paper only and is therefore very intangible and correspondingly safe so far as the author is concerned.

TABLE DIMENSIONS, &c., CROCKER-WHEELER MOTORS.

Horse Power	Net weight lbs.	Speed	Pulley "P." Diameter ins.	Pulley "F." Face ins.	Size of Motor over all "L." Length ins.	Size of Motor over all "B." Breadth ins.	Size of Motor over all "H." Height ins.	"G." Length between bolt holes in base	"E." Breadth between bolt holes in base	Diam. Shaft ins.	"D." Diam. Armature ins.	"C." Height of shaft centre from base of Motor
1/12	18	2,100	Grooved, 1 1/2 or Flat, 1 1/2	3/8 1	7 3/8	5 1/2	7 7/8	4 5/8	2 3/4	1/4	3 3/8	5 11/16
1/8	26	2,000	Grooved, 1 1/2 or Flat, 1 1/2	3/8 1	9 3/4	7 1/2	8 1/2	6 3/8	3 3/8	3/8	3 11/16	6 1/8
1/6	26	1,800	2 1/2	1 1/4	9 3/4	7 1/2	8 1/2	6 3/8	3 3/8	3/8	3 11/16	6 1/8
1/4	65	1,500	3	2	14 3/4	9 1/2	10 3/4	9 3/4	4 5/16	9/16	4 3/4	7 1/2
1/2	100	1,350	3 1/2	3	18 1/4	11	13	11 1/2	5 1/2	11/16	5 3/4	9
1	157	1,050	4	3 1/2	19	13 1/4	15 1/2	12 1/4	7	7/8	7 1/16	10 1/2
2	290	1,050	6	3	25	15 5/8	18 3/8	18 1/4	9 1/4	1	8 1/4	11 7/16
3	300	1,000	6	4	26 1/4	15 5/8	18 3/8	18 1/4	9 1/4	1	8 1/4	11 7/16
5	485	1,000	7 1/2	4 1/2	28	18 3/4	21	18 3/4	9 3/4	1 1/8	9 1/4	12 3/4
7 1/2) Special.												
10) Special.												

Discussion.[1]

The Chairman:—Gentlemen, you have all heard this very practical and instructive paper of Prof. Crocker's and since great pains have been taken to print the papers beforehand, or at all events so many of them as we have been able to get hold of, it is evident that most of you must have read it and that therefore the "reluctance" and "impermeability" of our magnetic circuits, which is usually so prevalent on occasions of this kind, will not prevail to the customary extent. We may expect therefore to hear a very liberal and full discussion, and remarks are now in order.

Prof. Anthony:—I would like to say just one word in regard to what Prof. Crocker has said in reference to joints in the field magnet. He certainly has not laid too much stress upon obtaining a large surface of contact between the wrought and cast-iron in forming a joint, and even in making a joint between two wrought-iron pieces it seems to me that a large surface of contact is needed also, for when we come to consider the matter any amount of machine work that it is practical to do economically does not produce anything like the surface of contact that you would have in a field magnet made up of one piece. For instance, suppose the pole piece and the core were united by means of a joint—I venture to say that with any joint that you could make there—that it would be practical to make there economically—that is, with any amount of work you could afford to spend, you would not have one-quarter of the surface of contact between the two metals that you have when you make it of a solid piece.

Mr. A. Langstaff Johnston:—My experience has been in street railroad work, and I would like to inquire, if the same laws would hold good in track connections. For instance, the contractor used a No. 6 wire, connecting it with a T rail. I condemned it for the reason that the surface was not equal to the size of the wire, the metal being so thin.

Prof. Crocker:—It was simply a question of contact, but electrical contact is a very different matter from magnetic contact, because we have no electrical saturation. Cast-iron will practically prevent lines of force from going through it at a point about 60 per cent. as high as the saturation of wrought-iron. Therefore a butt joint between cast and wrought-iron actually limits the number of lines of force to 60 per cent. of what the wrought iron would carry. Whereas in electrical conductivity we have no such fact It simply introduces resistance, but does not actually shut off the effect. Therefore this fact is particularly important in magnetic joints because we have magnetic saturation—a practically fixed limit, which we do not have in electrical conductivity. Where it is simply a question of elec-

1. By Messrs. Anthony, A. L. Johnston, Crocker, Hammer, E. P Thompson, Boynton, Mailloux, Wheeler and J. Martin.

trical contact of metals the usual rule is that the surface of contact should be several times the cross-section of the conductor. If a soldered joint, it ought to be 3 or 4 times; if a rough joint where the surface is oxidized, it might require as much as 10 times. Was it a tight fit in the case mentioned?

MR. JOHNSTON:—Yes, sir; driven in with a hammer on the side.

PROF. CROCKER:—It is hard to fix exact limits, particularly when the nature of the surface is not known and may be oxidized or not.

MR. JOHNSTON:—Consider that the surfaces are bright?

PROF. CROCKER:—I should, without considering particularly, say that the joint you speak of was not a good contact. It was not sufficient. But this question of uniting magnetic surfaces is very much more serious than uniting electrical conducting surfaces on account of the limit of magnetic saturation.

MR. W. J. HAMMER:—This question of contact in the fields of a motor calls to my mind some experiments I made at one time in connection with one of the old "Jumbo" Edison machines. As many of the gentlemen here will probably remember, the old machines had a large number of field cores—twelve, I believe—and it used to be a very difficult thing to get perfect contact, although the surfaces were scraped and polished. When the cores were bolted together there were consequent poles generated and I made some experiments at one time with a field explorer made from an incandescent lamp. I brought it up to a very high candle power—until there was this blue fluorescence all through the globe, which is very sensitive to magnetism—and I found it was a very sensitive field explorer and showed not only the strength of the field in different parts of the poles, but also the magnetic leakage. In passing it over the pole pieces of the machine I found out that there was quite a difference at the points of contact between the yokes and the various cores (making a sketch). You may remember they used to have a double row of fields below the armature, all connected to one pole, with four cores in each row and one row of four cores above the armature, and where they were bolted to the keepers the surfaces were polished very brightly and tightly fitted together. In some of these places where the bolts were not tightened up I found considerable difference in the lines of force and strength in the consequent poles formed. There was doubtless some little difference in the efficiency of the fields by tightening up those points, and that was indicated by this sensitive form of the field explor r.

MR. EDWARD P. THOMPSON:—With reference to the contact between the cast-iron and the wrought-iron, Prof. Crocker is certainly right in saying that there must be a large amount of surface of contact and that the distance between those surfaces must be a minimum, to such a degree and proportion as to make up for differences of permeability of the cast and wrought-iron. Now,

in order to get the same amount of surface the butt joint would need a flange or something of that kind. That would occupy a great deal of magnet coil space. It seems that the advantage that he pointed out then would be that he takes that flange, as you might say, and puts it in a hole where he takes out some of the cast-iron. In that way he gets the same amount of surface as with the flange and gets close contact and yet occupies less space simply because he takes out some of the cast-iron and puts the wrought-iron in its place. By means of inserting, as one puts a wheel on a car axle, the contact would be closer than you could ever get by any bolts.

MR. EDWARD C. BOYNTON, JR.:—I would like to add one feature of my experience to the discussion about the joint between wrought and cast-iron. Two years ago I tried the wrought-iron washer spoken of by Prof. Crocker, on the end of a rough wrought-iron field core to increase the area of contact, and butted it against a cast-iron pole piece. In order to reduce the expense, I afterward took off that wrought-iron washer and made as true a surface as possible at the end of the field core and brought as high a pressure as I could to bear upon that point, and with as careful tests as I could make at that time, I could find no difference in the efficiency of the machine whether built one way or the other—*i. e.*, with or without the washer.

I would like to say one word about the magnetic circuit and the amount of copper in the fields. I think, as regards multipolar machines, that the amount of copper depends a good deal upon whether you are using consequent poles or not. There is a machine at present in this city which develops 20 horse-power with 150 pounds of copper on the field magnets. I think it is obtained by the method used of multiple arcing the magnetic lines through the armature. I do not think that the above result can be reached by any bi-polar machine. Perhaps it can.

MR. C. O. MAILLOUX:—It might be well to state, in regard to the question of magnetic joints, that the matter was carefully and somewhat exhaustively gone into in Europe about a year and a half ago The results of the investigation were reported in the scientific press and it was found that the character of the contact made a great difference so long as we confined ourselves to rather low magnetic densities, but when once the density became high the effect, or defect, of the joint was not very perceptible. In other words, when once we reach saturation or any density approaching thereto, the nature of the joint does not any longer make very much difference. These two points are of some importance, and as the results were gone into carefully I think they are worthy of note.

The point noted by Prof. Crocker as to the usefulness of armature teeth in producing a retaining point on which the wires can act to pull the armature, is one of importance. We know that the current acts primarily upon the wire and not the core

of the armature, and that consequently it tends to displace the wire rather than the core. The armature core of a motor indeed only moves because the core is linked or held mechanically to the winding. In an ordinary smooth core armature the mechanical grip depends upon the tightness of the bands without which the winding would slip upon the core, as does sometimes occur when the bands become loosened from any cause. This point is of the more importance the slower the speed of the machine. As we reduce the speed of the machine we must increase the torque in order to maintain the same given power output. I have recently designed machines where the peripheral pull is as much as 2,400 pounds on the armature periphery at 400 turns per minute. One can readily see there that it would be almost impossible to undertake to confine the wire and to be sure that it would remain in the same relation with the armature core, unless the armature is laid in slots or otherwise securely fastened mechanically to the armature core

The point made by Prof. Crocker in relation to the lamination of pole pieces is, I think, very well taken. It has been my experience that the laminating of the fields and pole pieces is not only a very expensive process, but a very unsatisfactory one, mainly for mechanical reasons to say nothing of the question of cost. It involves mechanical expedients which are not necessary when you use solid cores and it makes the pole piece rather uncertain or unstable—one is never sure it will stay where it is put.

The eccentricity or "ellipsity" of the field space is another important point and it has one advantage which I think Prof. Crocker did not mention, namely, the fact that it tends to confine the lines of force within the sphere of the field space rather than to allow them to crowd toward the corners. It is a well-known fact that the lines of force are crowded by the action of the current toward the "entering" horn of the pole pieces of the motor and the "following" horn of the pole pieces of the dynamo; it is precisely for this reason that the horn of the pole piece, at which the armature enters when run as a motor and leaves when run as a dynamo, is always that which gets the warmer of the two horns. Now, the effect of making the field eccentric or elliptic is to increase the magnetic reluctance at the horns and it consequently tends to prevent and counteract the tendency of the lines of force to crowd at any particular part of the pole. It prevents the tendency to increase the magnetic density at any particular part of the pole and consequently it reduces the tendency to Foucault currents. It also equalizes the potentials from the positive to the negative side of the commutator and hence gives a better "Mordey" curve. As there is not so much action near the neutral point the sparking is greatly reduced.

With regard to the trapezoidal teeth, I quite agree with Prof.

Crocker that there is an advantage in endeavoring to increase as much as possible the peripheral surface of the armature core for obvious magnetic reasons. There is perhaps one point to which he has failed to call attention and that is the relation which the shape of the teeth and their form, etc, bear to the sparking of the machine. I have had considerable experience with Paciuotti teeth armatures and I have noticed that the shape of the teeth, their number, also the "entrefer" space or the "clearance," have an evident relation to the sparking. Mr. Esson in England has indeed recently pointed out that there is a relation between the armature current carried, or the current "sheet" of the armature, and the space which must be allowed in the air gap in order to eliminate sparking under heavy loads, the limiting current of the machine being different for the same air gap when the armature has teeth and also for different forms of teeth. The explanation which Prof. Crocker gives of the action of that peculiar form of tooth is perhaps not as full as it might be. Judging from my experience and study of the subject, it is my opinion that the form of teeth which he has described not only has the mechanical advantage but it has also a direct influence in reducing sparking; that with such a small number of teeth as he is using it would not be safe for him to make these slots of the ordinary proportion because he would doubtless find that he had very serious sparking and also heating of the pole pieces. The sparking phenomenon is connected directly with the dragging of the lines of force to the horn of the pole piece; whenever they are released in large numbers we get a very material amount of sparking and heating of the pole piece. The narrower the space between the teeth the more uniformly the magnetic lines are distributed. You perhaps know that in the Oerlikon machine, built in Switzerland, the problem of reducing the reaction of the rebounding lines of force has been overcome in another way, which consists in making holes near the periphery of the armature core and passing the wire through them, the winding being, so to speak, imbedded in the core, which revolves close to the field poles. I have understood (having seen only one myself) that in such machines there is very little sparking, in spite of the fact that they have a very small clearance. It would seem as if the relation admissible between the volume of current around the armature, as Mr. Esson terms it, or the current "sheet" of the armature as I term it, and the clearance space is much different in the case of such an armature and allows of the current being greatly increased.

The Wenstrom machine, as you know, has a similar form of armature periphery. Instead of putting one single wire through, the openings are fewer in number and larger, and hold a larger number of conductors. I have tried experiments on the subject using ordinary forms of slots—the square slots—and I have endeavored to attain approximately the results which Prof. Crocker

has obtained by his trapezoidal form of tooth in another way. The machines were a pair of four-pole tram car motors with solid wrought-iron fields. I was aware that unless the pole pieces at least were laminated I would get some heating action at the pole pieces. As I wished to find out the amount of heating I first tried the motors in the ordinary way. I then took off the armature bands and put on bands made of tinned iron wire. I made these iron wires very narrow and placed them close together on the armature. These tinned iron wires practically closed over the space between the contiguous teeth or slots at several points. I found that this made a very decided difference not only in the operation of the machine so far as sparking was concerned, but also in the heating of the pole pieces, both of which it reduced materially. I also tried the same expedient with ordinary armatures having smooth cores. One machine in particular with which the experiment was tried was an old high speed machine whose magnetic and electrical proportions were evidently wrong. At any rate, it had the vice of sparking to such an extent, and its use was so unsatisfactory even at light load, that it had been practically discarded. As a matter of curiosity more than anything else, the bands of brass wire were replaced by iron wire bands. Quite a serviceable machine was the result of the change. The sparking was reduced greatly and the "limiting" current was increased considerably by that simple expedient. I account for this by reasoning that the lines of force at the horn of the pole pieces oscillate or bend to and fro in smaller numbers and over a shorter distance when the tooth is partially bridged as in Prof. Crocker's form of tooth or when an iron wire band is used, or yet better, when wholly bridged by magnetic substances of the armature core itself as in the two types just noted. I should therefore expect the motors now before us to have the quality of a high "limiting" current, that is to say, of being able to carry a larger excess of load without injurious sparking than in the case of ordinary toothed armatures.

Prof. Crocker speaks of the efficiency of motors being necessarily smaller when the speed is reduced. That is doubtless true if we make the weight constant, or attempt to limit it, but if we do not entertain the weight in that relation it does not necessarily follow that we lose in the efficiency by reducing the speed. I think the machine can be made as efficient at a low speed as at a high speed if one be willing to make it larger, because a motor involves three variable quantities, one of which is the number of lines of force. If we are willing to increase that factor, which means to increase the quantity of iron and copper, I see no reason why the efficiency may not remain constant for a given capacity at lower speed.

In conclusion, there is one point that I would like to speak of, not exactly as a criticism of Prof. Crocker's paper, but more as a suggestion which I hope will be followed further. I would have

liked to have seen in Prof. Crocker's paper more attention paid to such data as might be useful to the engineering profession. In mechanical engineering, as you all know, we reduce everything to formula. We have formula, empirical or other, giving the length of a shaft as a proportion of its diameter, for a given speed, torque and distance between bearings. And so with many other parts of machines, the proportions of which can be arrived at by a simple formula based upon collated experience. I notice that in Europe there is a tendency to likewise assimilate electrical knowledge and put it into digestible form so as to make it more available and convenient for the practical engineer.

Consequently it would have been interesting if Prof. Crocker had given us such things as the ratios of watts dissipated to surfaces on which they were dissipated and the speed of moving surfaces. He dwelt largely on the per cent. of heat wasted in the field. I submit that these data are not of as much value to us as they would be if they were taken in relation to the extent and character of the surfaces in action, because in Europe, particularly in England, they have already begun to arrive at some definite system or rule in the proportions. They have reduced the mechanical proportions to a sort of standard; for instance, the ratio of the diameter of armature to radial depth of core and to the peripheral extent or width of poles, the magnetic density in the field cores, entrefer and armature, and many such things which are of great utility and convenience while at the same time they do not at all disturb secrets of manufacture. The same matter is possibly stated in the paper, only it might have been stated in a more accessible form, tabulated or formulated, so that it could be used more conveniently by the practical engineer.

Mr. Boynton:—I would like to refer to one point in regard to the sparking effect as governed by the area of the tooth. I have seen a number of experiments with different sizes of teeth and the figure arrived at is that if the area of the tooth on the periphery is equal to the area of the core of the field magnet that the sparking will not be noticeable. I do not know how far one way or the other you can go from that figure without bad results, but it is safe to use it. In the armature of Prof. Crocker I should say that the distance from the bottom of one of these trapezoidal slots to the bottom of the next should be equal to the area of the core, very nearly.

Dr. Schuyler S. Wheeler:—Does the gentleman mean the area of the sum of the teeth that supply one side of the armature or of only a single tooth?

Mr. Boynton:—The sum of the areas of the number of teeth under the pole piece at one moment.

I would also like to state a point in regard to eccentricity of the curve in pole pieces. That can also be done in another way. The curve can be allowed to follow the armature almost the whole extent of the pole piece and then be gradually backed off to the points.

DR. WHEELER:—I would say that I hardly expect to see a properly constructed dynamo or motor made, in which the area of the sum of the teeth under the pole piece is less than the cross-section of the armature, because in that case the magnetism would be throttled by the teeth.

MR. MAILLOUX:—That is a matter which is self evident, and it has been pointed out in the discussion of Mr. Esson's paper before the Institution of Electrical Engineers. It was shown that in machines using toothed armatures the combined area of any teeth under the pole ought not to be less than the section of the armature core itself. If it is made less, the magnetic density is greater and the permeability is correspondingly lowered which means that the ampere-turns of the exciting coils must be made greater. After all, the whole question mainly is, how many ampere-turns one is willing to devote to this portion of the magnetic circuit. With an air space the permeability is unity Even if we were to crowd the lines of force in the teeth to a density of 30,000 or 40,000 per square centimetre, we would still have a permeability of twice to four times that of air. Of course, there are other reasons besides the increase in exciting energy required, as, for instance, the distortion of field, which render such extreme densities undesirable It is a fact, however, that high densities, say 25,000 or more, can be used in the teeth in special cases.

MR. J. MARTIN:—In reference to the contact surfaces of the cores and yokes I would state that the earlier practice of the Edison company was to scrape them as accurately as possible. In the language of a friend of mine who did considerable of this work, "a slide valve was nothing to it."

That practice has been abandoned and the present machines are turned out with the surfaces simply machine tooled. In testing a machine lately which did not quite realize anticipations, I found that the assembling had been done rather carelessly and considerable dirt was between the adjusting surfaces. I cleaned them carefully, rubbing them with white paper until no discoloration was apparent. On reassembling and repeating the saturation test no practical difference from first test was found.

This machine had a smooth core and the gaps required about 80 per cent. of the whole magnetizing force. The tests on the effects of joints alluded to by Mr. Mailloux are quite fully reported on in Silvanus P. Thompson's book, "Lectures on the Electro-Magnet."

In my designs for machines I have always allowed, as stated in the tests just mentioned, for the resistance of the joint as an air gap of from one-half to one-thousandth of an inch in length depending on the density of lines at the same. However, it is a matter hardly worth taking into account when the surfaces are of like material and reasonably well surfaced. As probably an extreme case of heating I would mention a test recently made on a shunt dynamo which was started at the point of maximum

watt output of its cold external characteristic (its capacity was stated to be about that which could be obtained at this point.) In three hours the output in watts had decreased exactly one-third. This was a small machine and not very efficient and I only mention it as showing one of the effects of heating.

Speaking of heating brings to mind the unreliability of measurements made with thermometers, of increase in temperature, especially in the case of armatures. Judging from my own experience and the observed practice of others, I am inclined to doubt the accuracy of results obtained by methods other than measuring the increased resistance and calculating the increase in temperature by the well-known formula. I would like to ask Professor Crocker or any other member who has constructed machines with armatures of the Pacinotti type, of any considerable size, if they can state the point at which there is a tendency in that particular form of construction which by the decrease in the magnetizing force required for the air gap causes the armature to become relatively too powerful for the field and so does not allow this feature in the construction to be utilized to its fullest extent. The "large output" machines devised by Crompton and Swinburne are, I believe, an example of my meaning and require external devices to render them operative.

I would ask if the shaping of the pole pieces, as to cross-section, of the Crocker-Wheeler motors is the result of experiment.

In one of perhaps an earlier type than the one exhibited I noticed what seemed to be an apparent increase in cross-section at a distance above the spools. I understand that in some of the smaller machines at least one coil is wound over the other in the same slot.

I have had some experience with Gramme armatures in which the coils were not symmetrical in either position or length, and there was uneven wear on the commutator. I notice that the outer tips of the poles are closer to each other than the inner, and would ask if this is the result of experiment.

There is a point in connection with multipolar machines which until mentioned by Mr. Swinburne in the discussion of Mr. Esson's paper, had not, I believe, been properly placed before the public. This is the unequal induction through the paralleled armature circuits, due to journal wear. In a multipolar machine with Pacinotti armature and small air gap, especially the wear which in commercial work always takes place and has no detrimental effects (within limits), in bipolar machines would result in considerable trouble. From information lately received, this has been the case in certain very large machines in Europe, which are being held up to our admiration, and has been provided for in some later constructions by placing the armature shaft vertically. It is a matter of some surprise to me that this point has not been provided for in the constructions which I have had opportunities for examining.

PROF. CROCKER: —I simply stated in a general way that a good joint was desirable, which I think no one will deny. Furthermore, the resistance of a joint, which I think is properly stated to be equivalent to an air gap of so many thousandths of an inch or as equivalent to so much of the iron circuit—the advantage of expressing it in terms of air gap is that air has a permeability of unity and is definite. I spoke particularly of the necessary surface of contact between wrought and cast-iron. If cast iron has fifty or sixty per cent. of the permeability of wrought-iron, it requires a corresponding increase of surface of contact. If there was a welded joint between the two, it would still need the increased surface of contact. Incidentally the construction I suggest secures a very good joint because the core is made exactly true and of exactly a certain diameter and so is the hole into which it fits, and it slips in with no looseness at all. As a matter of fact, it fits within a fraction of a thousandth of an inch and I think this is desirable, particularly where the whole magnetic circuit is of low reluctance; in other words, a given amount of reluctance in a joint, whatever it may be, will have more effect in a short magnetic circuit of small reluctance than it will in the ordinary magnetic circuit with a half-inch air gap. The results recorded where joints have no effect is usually, I think, where they have had a large amount of magnetic reluctance either in the magnetic circuit or in the air gap between the pole pieces and armature. I have made a test in which you could slip a visiting-card into a magnetic joint as a result of the presence of dirt, and yet when I made the joint perfect it made no perceptive difference because the rest of the magnetic circuit had such a large reluctance that the poor joint was of no consequence. But if the rest of the circu t had a small reluctance, it would be of great consequence. When we get, in a few years, to look at magnetic reluctance as we now do at electrical resistance, then this whole question of joints will take its proper place.

In regard to the pole piece being brought nearer on the upper side, I would say that the object of that is to counteract the tendency of a greater number of lines of force to go through the lower side. I find that on account of its being a shorter path, we get 50 per cent of the lines of force through the upper half of the pole piece by this plan. This also tends to lift the armature and reduce the friction due to its weight. If the upward magnetic pull was equal to the weight it would reduce friction to a minimum.

In regard to the peculiar form of the pole piece—the plough form. The object is to make the teeth come into the field gradually. It is the same point that Mr. Mailloux spoke of in regard to flaring the pole piece, which we always do. Pointing the pole piece and flaring it both have the effect of making gradual the entrance or exit of a coil or tooth and I think this quite important, particularly in toothed armatures. What

Mr. Mailloux said agrees exactly with my experience, that toothed armatures should have a flaring pole piece and should have a pointed or rounded pole piece also. It is also desirable on account of reducing sparking and keeping the lines of force in the middle of the pole piece rather than have them pile up in one corner of it. Another thing is that if there is a perfectly square edge and a perfectly square tooth it will produce a hum due to the sudden effect produced when the edge of the tooth comes opposite the edge of the pole piece, but if the edge of the pole piece is inclined, the passage is gradual and the hum is reduced.

In regard to two sections in one slot, I would say that we do equalize them and I expected a tendency to spark in one of them more than in the other. That is to say, I expected to have sparking of the commutator at every second bar, but it does not seem to occur. Of course, if two sections are in one slot, that means that between one pair of sections there is no tooth; between the next pair there is a tooth which apparently ought to have an effect, but it doesn't, and I attribute it to what Mr. Mailloux spoke of—the continuity of the magnetic surface. We have practically a continuous magnetic surface of armature core and therefore when a tooth comes in or out of the field it does not do so suddenly. I confess I was quite surprised when we were able to bring two sections into a slot without having sparking; I expected more.

Mr. Mailloux :—If I may be allowed to take another minute, I would like to say that in regard to putting two sections in one slot, that if they were not superposed—that is, if they were simply opposed or set side by side—the sparking would probably be less, simply because the arrangement prevents the lines of force from rebounding through the whole space at one operation, but subdivides the jump into two portions.

In regard to the equivalent air resistance of a joint, I think the matter can be put more clearly in this way: Between two wrought-iron surfaces or between two cast-iron surfaces I think we can deal with the resistance as if it were an air resistance, but if when dealing with a wrought-iron and cast-iron contact in which the wrought-iron happens to be the smaller of the two, I do not think that you can treat it as if it were an air resistance. Let us assume, for instance that we have a rod of iron making a butt joint and that the rod has a square centimeter of section. Now, if the section of the iron is only one square centimeter, then with a high density, say 20,000, it will bring down the permeability of the wrought-iron to say 30 or 40; whereas in the cast-iron the permeability will be almost literally nothing—two or three perhaps; possibly even less. So that not only have we to take into consideration air resistance, but we have to consider the immensely increased magnetic reluctance due to the greater density per square centimeter in the cast-iron itself, because the cast-iron will not admit of such density as the wrought-iron.

The matter of armature connections in multipolar machines has been discussed by the gentleman who spoke previously. It is a well-known fact that multipolar machines require certain precautions which are not needed in bi-polar machines. In bi-polar machines you have only one magnetic field. In multipolar machines you have two, four or more. If no special precaution is taken in connecting the armature circuit, it follows that we must have the magnetic fields very accurately balanced, so that they may be all equal to each other. If we do not, whether it is by allowing the armature to come closer to one set of poles, as in the case referred to by Mr. Swinburne recently in England; whether it is that we do not have the same magnetic resistance in the core itself, or whether we have not the same number of turns, or from any cause, the exciting current passing through some of the magnet coils is reduced—if in any way we disturb the equilibrium between the magnetic fields, it is evident that some of the many circuits in parallel will be developing a higher E. M. F. than the rest. I have had experience with six polar machines, where connections were made in the usual way, using say six brushes and consequently having six parallel paths for the current in the armature, and I have found that if anything happened whereby a pair of poles were magnetically short-circuited or anything that would destroy the magnetic balance, the disturbance might be so great as to burn the armature, simply because one portion of the armature then has to revolve in a weakened or dead field, and as it generates little or no E. M. F., it would be simply the path forming the short-circuit between the other sections and consequently would be rapidly burned up This fact was not pointed out first by Mr. Swinburne. Mr. C. S. Bradley wrote an article in the *Electrical World*, in March, 1887, in which he pointed that out very clearly, in connection with the description of his winding for multipolar machines. The matter has been perfectly understood by all engineers who are at all familiar with the design and construction of multipolar machines, and I may say that there is no excuse to-day for anybody running into that error, because several windings have been invented which entirely obviate the troubles from unequal magnetic fields I am now building motors with as many as twelve poles and it would be simply suicidal to fol low the old method of winding, to say nothing of using twelve brushes. In such cases some other way must be followed of overcoming the difficulties and insure our having an equal action over every part of the armature.

I think it is fitting, perhaps, to say that one thing we must admire in Prof. Crocker's paper is the considerable amount of what we are wont to term in this country, "horse sense," manifested in the author's ideas and opinions on this subject. I think it is quite refreshing that we are at last entering on a phase where the electrical engineer is superseding the inventor, where the necessity for paying strict attention to electrical and mechanical en-

gineering requirements is becoming obvious to all of us, and where, as he has clearly pointed out, the question of the shape of motor, the question of a fanciful design of the armature as such, has become a question of good common sense, coupled with engineering knowledge and skill.

THE CHAIRMAN:—The time approaches when we must take our recess. I would like to say before closing this discussion, that I have been extremely pleased by the practical turn it has taken and especially with the few remarks with which Mr. Mailloux has closed. To my mind the practical nature of the paper is its great merit, and, although Prof Crocker says it has but one merit, namely, that which may be found in the doxology at the end of his paper, I am quite sure we have found many in it. It is too frequently the practice in America to bring the two terms "practical" and "practicable" into harmony and use them as synonymous terms. It is certain that in the experience of many of us, things practicable have been found which were totally unpractical.

The discussion seems to have concentrated mainly on the importance of having a perfect magnetic circuit. I do not know that that was the design; in fact I am quite sure it was not the design of the author of the paper to emphasize that, so much as to emphasize the necessity of the perfection of joints between cast and wrought-iron. I have been specially interested in one little remark which did not seem to attract general attention, namely, that the supposition that cast-iron is cheaper than wrought-iron, is a fallacy, its permeability being as much below wrought iron as is its price, both being about one half.

While I am in entire accord with the spirit of that paragraph I would venture to so far depart from its terms as to say that cast-iron is cheaper than wrought-iron and *therefore* it is not advantageous to use it. [Applause.]

Some years ago at the suggestion of our Past-President, Mr. Franklin L. Pope, I wrote an article entitled, "The Cheap John in Electricity." Perhaps it has had as wide a circulation as anything I have had the misfortune to write, but it is the one thing that I have written of which I am the least ashamed. I do think that cheapness should be obliterated from electrical considerations. Substitute for the word cheap, economy, and it is all right; but there is no economy in using cast-iron instead of wrought-iron even though its price be one half. As I am afraid I cannot be with you this afternoon, and as Mr. Hammer is here I will ask him to preside.

You are invited to take a collation, which is prepared, and however much many of us may dissent from the propositions of Prof. Crocker and Mr. Mailloux, I am sure we shall heartily be in accord with one statement of both gentlemen, namely, that with respect to the usefulness of teeth.

RECESS.

A paper read at the General Meeting of the American Institute of Electrical Engineers, New York, May 20th, 1891. Vice-President Hammer in the Chair.

A PHOTOGRAPHIC STUDY OF THE ELECTRIC ARC.

Based upon Experiments made by Mr. J. C. McMynn.

BY EDWARD L. NICHOLS.

At the last General Meeting of the Institute, Mr. Louis B. Marks read a paper on the electric arc, which dealt with the performance of commercial lamps in the continuous current circuit. The present paper is a preliminary report of a similar line of work now being carried on under the writer's direction, by Mr. J. C. McMynn; the subject of the investigation being the alternating current arc When the research in question is completed, Mr. McMynn will publish the results in full; but, since his report may not be ready for several months, it seemed desirable that I should lay before the Institute at this meeting certain points which have already been established.

The alternating current arc and the continuous current arc afford two entirely distinct fields of investigation. Candle-power, distribution of light, quality of light, temperature, efficiency, resistance, electromotive force necessary to maintain the arc, performance of the carbons under varying conditions; all of these are questions concerning which we have abundance of experimental data, so far as the continuous current arc is concerned. In the case of the alternating arc, however, these and many other points are still to be determined.

The arc lamp, fed by alternating currents, invariably hums and sings. The note, which is not to be mistaken for the hissing which we hear from continuous current lamps that are burning under improper conditions, possesses a definite pitch, determined by the rate of alternation of the dynamo. That the note is not due to the mechanism of the lamp itself is readily shown. The

Jablochkoff candle, which is devoid of all mechanism, sings as merrily as the lamp with solenoid regulator; any form of hand-lamp will do the same. I do not remember having seen any definite statement concerning the source of this humming noise, nor any satisfactory discussion of the question. My attention

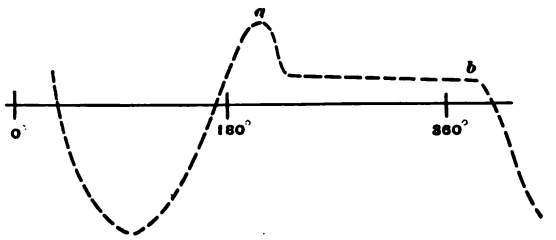

FIG. 1.—From Archbold and Teeple's thesis on "The Ball and Point" effect.

was first brought to the very obvious explanation of the phenomenon by the study of certain curves of electromotive force, obtained by Messrs. Archbold and Teeple, in the course of an investigation of the alternating discharge between a ball and point[1]. These curves have recently been published. I reproduce one of them for the purpose of elucidation[2]. This curve was obtained by the well-known method of instantaneous contacts. The circuit under investigation was the high tension secondary of an induction coil, actuated by an alternating current dynamo, the E. M. F. of which followed the law of sines closely. In the circuit were placed a ball and point, the air space between them being such that the discharge would take place in one direction only, from ball to point. It was established, by the use

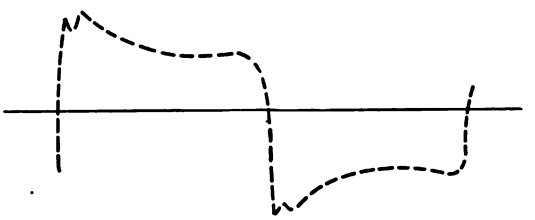

FIG. 2.—From Tobey & Walbridge's paper on the Stanley Arc Lighting Machine.

of a revolving mirror, that an arc existed only during that part of each cycle represented by the horizontal portion of the curve.

1. See Archbold and Teeple; Thesis in MS., Library of Cornell University, 1889.

2. See "The Alternating Electric Arc between a Ball and Point." *American Journal of Science*, Vol. 41, p. 1.

[*a*—*b*, figure 1.] Shortly after this curve had been obtained, the curves of Messrs. Tobey and Walbridge, illustrating the performance of the Stanley arc lighting dynamo, appeared. It will be remembered by those who heard their paper read before the Institute, that this machine, which gave very nearly the sine

FIG. 3.—Alternating Arc. [Movement of plate horizontal.]

curve of E. M. F. when working through non-inductive resistance, showed curves very widely removed from that form when arc lamps were in the outer circuit.[1]

Figure 2 contains the curve of the arc-lighting machine when five lamps were in circuit. The relation of this curve to those obtained with the ball and point is evident. Here, also, the variation of the curve from sinusoidal form is to be ascribed to the periodic formation and breaking down of the arc, which happens twice in every complete cycle, instead of once, as in the case of the "one way" discharge of the ball and point phenomenon.

After studying these curves it seemed to me unquestionable that the humming of the alternating arc is due to the rapid periodic extinction and re-establishment of the discharge, the effect

FIG. 4.—Alternating arc. *a*. Long arc (58 volts). *b*. Short arc (43 volts) [Movement of plate horizontal.]

being similar to that observed in singing flames. When, some months later, opportunity was found of viewing the image of the

1. Tobey and Walbridge: TRANSACTIONS, Vol. 7, p. 367.

arc of an alternating current lamp in the revolving mirror, I was prepared to witness the extinction of the arc during a definite part of each alternation. I do not know whether any member of the Institute has tried this simple experiment. I have seen no description of it anywhere, although Wiedemann mentions the

FIG. 5.—Alternating arc. [Movement of plate horizontal.]

application of the revolving mirror to the study of the continuous current arc[1], and Joubert[2] (1880) makes brief mention of a very ingenious application of the plenakistiscope to the study of the changes to which the alternating arc is subject, during each period. When such a mirror, mounted upon a vertical axis, is set up at a convenient distance from the lamp, the images of the white-hot tips of the upper and lower carbons are drawn out by the revolution of the mirror into uniform, parallel bands, extending quite across the field of view. The intervening narrow strip, much darker, is the image of the arc itself. This region, however, is not uniform. It consists of alternate patches of purple and black, which mark the intervals during which the arc exists and is extinct.

Mr. McMynn's first step in the investigation which it is my purpose to discuss in this paper, was to photograph the arc under conditions which would bring out this phenomenon. A camera

FIG. 6.—Alternating arc. [Movement of plate horizontal.] High speed.

with long bellows was set up near the lamp, the distances from plate to lens and from lens to lamp being such as to give a some-

1. Wiedemann; *Elektricitaet*, 4, p. 855.
2. Joubert, *Journal de Physique*, Vol. 9, p. 390.

what enlarged image of the arc. In the focal plane an opaque screen was mounted, which contained an opening just large enough to receive the image of the carbon points. The apparatus having been adjusted, a sensitive plate was driven past the aperture at high speed. The photographic image differed from that observed in the revolving mirror chiefly in the relative brightness of the arc proper, and of the incandescent carbon of the points. To the eye, the latter is the dominant feature; whereas the *actinic* image of the arc far exceeded in intensity that of the carbons.

These photographs establish the intermittent character of the alternating arc beyond question. They also exhibit in a new

a. *b*

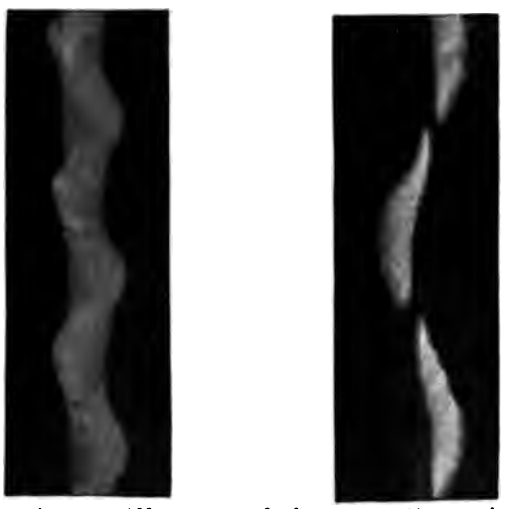

FIG 7.—Alternating arc. [Movement of plate vertical]. *a.* Showing oscillations due to earth's field. *b.* Showing oscillations in a stronger field.

light many interesting properties of the arc, some of which are already well-known, while others have not been noted. Of the many negatives already obtained, a few characteristic forms are given in the accompanying plates.

Figures 3, 4, 5 and 6 show the results obtained with plates moving in a horizontal direction, at right angles with the axis of the carbons of the lamp. Figure 3 is an enlargement of one of the first negatives taken. The speed of the plate was moderate, the size of the image smaller than the arc itself. The plate contains but little detail The carbons do not appear at all in the photograph, which shows little besides the periodic interruption

of the arc. In later negatives the size of the image was greatly increased and the plate was driven much more rapidly through the field. In many instances two or more successive exposures were made upon the same negative. Inspection of figures 4, 5 and 6 shows that the greatest intensity resides neither in the arc itself nor on the incandescent carbons, but in a narrow region just at the ends of the arc. In this region, which, doubtless, is that where the so-called "contact resistance" of the arc occurs, the illumination is sometimes intermittent as in figure 4; sometimes nearly uniform, as in figures 5 and 6. The relative width

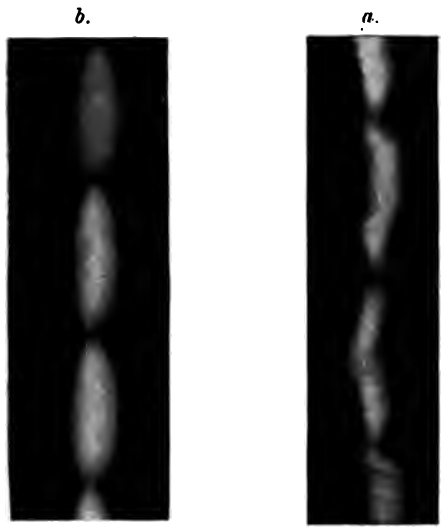

FIG. 8 —Alternating Arc between cylindrical carbons. [Movement of plate vertical.] *a*. Arc viewed from North showing oscillations due to earth's field. *b*. Arc viewed from West.

of the intervals of darkness and of light is also seen to vary greatly under conditions not yet determined. Figure 6 represents the highest rate of speed given to the plate in any of these exposures. The image of a single alternation was in this case extended almost across the plate. No new features were brought to light, however, by this process of magnification.

Figures 7, 8, 9, 10, 11 and 12 were obtained by driving the plate vertically through the field, parallel to the carbons. An unlooked-for feature of all these plates was the oscillation of the arc from right to left in synchronism with the alternation period. Mr. McMynn suspected this vibration to be due to the action of

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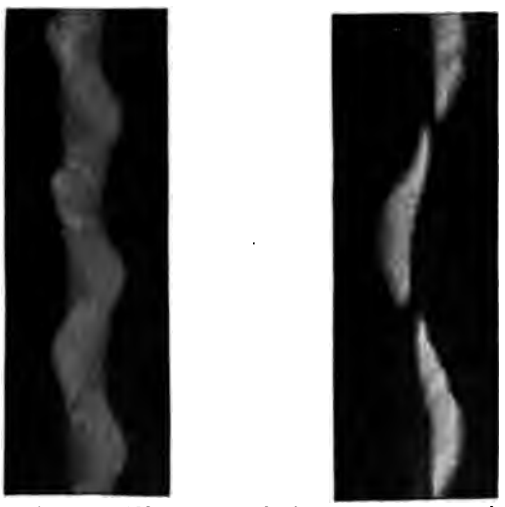

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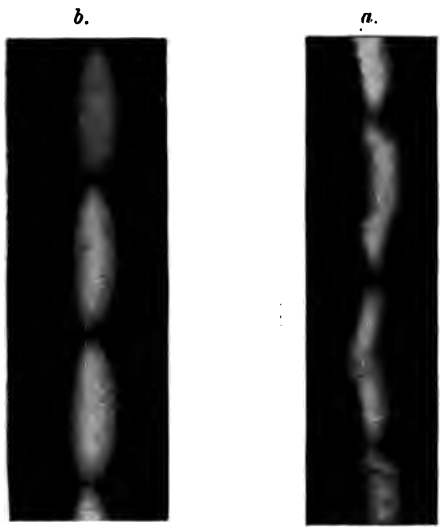

FIG. 8 —Alternating Arc between cylindrical carbons. [Movement of plate vertical.] *a*. Arc viewed from North showing oscillations due to earth's field. *b*. Arc viewed from West.

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Figures 7, 8, 9, 10, 11 and 12 were obtained by driving the plate vertically through the field, parallel to the carbons. An unlooked-for feature of all these plates was the oscillation of the arc from right to left in synchronism with the alternation period. Mr. McMynn suspected this vibration to be due to the action of

the earth's magnetic field[1]. He was able to verify his view by comparing photographs made with the camera placed with its optical axis in the meridian with others in which the arc was viewed from the east or west. The disturbance was most marked in the former position and disappeared almost entirely from negatives taken in the latter. This point is well illustrated by figure 8 (*a* and *b*), which shows the image obtained from the north and from the east. The lamp in this case was supplied with cylindrical carbons. All the other photographs represent the arc between flat carbons. In a strong field, produced by

FIG. 9.—Alternating arc. [Movement of plate vertical; arc in strong magnetic field.]

FIG. 10.—Alternating arc. [Movement of plate vertical; arc in strong magnetic field.]

placing the arc nearly between the poles of a horse-shoe magnet, the lines of force being perpendicular to the axis of the lamp and parallel to that of the camera, the vibratory effect was much increased. [See figure 7 (*b*)]. The bearded appearance of the arc in figures 9, 10 and 11 is common to all the negatives taken while the arc was in the strong field. The spines are supposed to be the paths of very minute and highly incandescent particles, expelled from the arc at a considerable velocity under the action of the magnetic forces. A continuous current arc, also in a strong field, the conditions being as nearly as possible those under which figures 9, 10 and 11 were taken, gave a negative of which

1. It should be noted that Casselmann, as long ago as 1844, described the deflecting action of the earth's field upon the continuous current arc. (See *Poggendorff's Annalen*, 63, p. 588.

figure 12 is a very imperfect representation. The arc was flaming, and it will be seen that while the path of the arc itself is an unbroken, straight trace, the flames, which show themselves to the right and left hands, are irregularly intermittent. The negative shows, moreover, that the flame to the right hand, in the figure, is finely striated, reminding one of the image of a hissing, sensitive gas flame, as the latter appears in the revolving mirror. This vibration, which must represent a pitch of several

FIG. 11.—Alternating arc. [Movement of plate vertical; arc in a strong magnetic field.]

FIG. 12.—Continuous current arc. [Movement of plate vertical; arc in strong magnetic field; arc flaming.]

thousand vibrations per second, is in all probability the cause of the hissing of the arc.

In this brief discussion I have left untouched many points which will suggest themselves to those who study Mr. McMynn's photographs. The interpretation of many of the features of these plates is at present rather obscure, but it is hoped that experiments now in progress will add something to our present knowledge of the phenomena connected with the alternating electric arc.

Physical Laboratory of Cornell University, May 9th, 1891.

Discussion.[1]

The Chairman:—The paper is open for discussion. This is a subject that is attracting a large amount of attention, and the Chair trusts that the discussion will be largely indulged in. Prof. Thomson is present with us this afternoon, and with his extensive experience in such matters I have no doubt we shall be very much edified to hear him.

Prof. Elihu Thomson:—There are but a few thoughts that occur to me. I have had the paper in hand only for a few moments. This did not happen to be one of the papers I got in advance. But I would suggest that a very good thing to try in this connection would be to actually produce, if possible, an arc with a sine curve wave; that is hold the sine curve wave by a large resistance in the circuit or by self-induction, and then investigate the characters in a similar manner. Of course the presence of the arc would probably deform the wave some. The reason for the curve of current taking the form it does is that, as the resistance of the arc falls with the increase of current, naturally the moment the fall begins, the current increases to a point only limited perhaps by the current capacity of the apparatus feeding the arc. The vertical line shown in the curve of current with the arc, shows that the moment the arc starts there is practically no limit to the current which might pass through that arc if the electromotive force remained constant. The curve indicated by Prof. Nichols would naturally be expected to be the form of an alternating current—the curve of an alternating current feeding an arc. But of course if in the circuit there should be a considerable resistance, with a considerable self-induction, the curve would naturally change; that is, the nearly vertical part would probably become more sloping and an approximation to a sine curve would result.

There is one matter in connection with the photographs that I may mention. It is that I do not think you can argue absolutely that the current stops or that there is an absolute cessation of the current in the arc because the photograph doesn't show it. It may be that the current flowing is so small as not to give actinic luminosity to the stream, but it is also well known that a photographic plate to develop strongly requires a certain amount of actinic work done on it. Taking the ordinary case of a sensitometer used to determine the sensitiveness of the plate, if you put over a plate such a sensitometer, consisting of a number of translucent squares, and expose it to light you will often find a strong development up to a certain sensitometic number, and after that a quick change. That is, there is a certain strong development after a certain amount of exposure, as if the plate was resistant up to a certain point and gave way when the light had reached the plate in a certain amount. It may be, as Mr. Mail-

1. By Messrs. E. Thomson and Hering.

loux suggests, a sort of coercive force in the plate. I am reminded of the photographs of electric sparks exhibited some few years ago.

Some cases of the photograph of a spark gave a reversed image, that is, a dark image on a comparative light ground. Here we have the case of another action of a photographic plate, which is solarization. Too much light will cause insensitiveness. Some of the results seen here may admit of such an interpretation. I notice in the lower part of Fig. 5 what seem to be little dark spaces in between the wider black spaces, and in Fig. 6 this seems rather startlingly shown, and the question here arises whether it is really due to a drop in luminosity, or whether it is due to solarization, but as the curve of current shows a dip there, it probably is due to drop in luminosity and not to solarization. Still this action of solarization would be a matter to be taken into account in all these cases. The results are certainly very interesting, and the field of work is one which is certainly of great interest to those connected with the application of arcs in practice. The results certainly show that the acuteness or sharpness of the sound—the noisiness of the arc—is increased by the fact that there is a tendency to an absolute break. We can readily imagien that if the curve of the current was a sine curve that the note given out would be a very smooth, musical note, but in the case before us, where the results seem to indicate almost an absolute break in the current we can understand that the effect is, as it were, a set of lashings or snappings in the air and the noise produced would be sharper and harsher to the ear in consequence. This harshness might probably be ameliorated by following the suggestion that I made at the start, as by actually forcing a sine curve current through, keeping the wave as near as possible a sine curve. [Applause]

Prof. Nichols:—I should like to say with reference to the remark of Prof. Thomson that the photographs were secondary in this case to observations with the revolving mirror; that I should hardly have ventured to interpret photographs alone because I am well aware of the difficulty of so doing. But a view of the arc in a revolving mirror makes it evident visually. All that can be said is that the mass is so very small that it doesn't take an appreciable time to cool below the point at which it gives out light. That is the meaning of extinction in the way I used it.

Mr. Carl Hering:—I would like to ask whether, in Fig. 12, the slide was placed east and west, or north and south in the magnetic field; also whether the current was absolutely steady like a battery current, or whether it was of a pulsating character like that from a dynamo. It seems to me that the effect might be quite different with a pulsating current, such as is generated by some of the prominent arc light machines, than with a battery current which is entirely free from pulsation.

PROF. NICHOLS:—I would say that Fig. 12 represents a continuous current arc in the same strong magnetic field which was used in Figs. 9, 10 and 11, so that the flaming on the side shows that there were two elements, one moving in one direction and one in the other It was viewed in the east and west direction and was under the influence of an artificial field. The lines of force might be considered as perpendicular to the paper and horizontal. The lamp was fed by a current from an Edison machine—a single lamp with resistance in series sufficient to cause it to regulate properly. There was probably rather a smooth current.

THE CHAIRMAN:—If there are no further remarks on this paper the Chair would like to state that Mr. Pope, the Secretary, has informed me that there is a matter coming up for consideration before the Institute which is not down on our programme, and I do not doubt that the Institute will be very glad to hear from Mr. Mailloux in reference to this matter.

MR. MAILLOUX:—Mr. Chairman and gentlemen of the Institute: There has been delegated to me a duty which is exceedingly agreeable were it not for the consciousness which I have that I am not quite adequate to carrying it out properly and performing it in a due and satisfactory manner. I think there is no member of the Institute who has been at all punctual in attending the meetings, who does not feel that the Institute has been especially fortunate in its presiding officer during the last year. We all felt that we had a man at the helm who was not only earnest in his endeavors to benefit and advance the welfare of the Institute, but who was known to set the example of punctuality by being present at every meeting, and whenever his presence was required, even at great personal sacrifice and under unfavorable circumstances, living as he did at a considerable distance out of town. Further, all must have noticed the impartial and business-like manner in which the affairs of the Institute were conducted at the meetings. Some of the members have felt that in that respect we owe something more than a vote of thanks to Prof Anthony, for the manner in which he has served us in the Institute during the last year, and one member particularly, who is too modest to make himself known, has put into practical form his appreciation of the services rendered by Prof. Anthony. It is to me a great pleasure to have the opportunity of expressing the admiration which I have always had for Prof. Anthony as a man, as our worthy presiding officer and as an American electrician, and I want to put the accent on the word electrician, because we should know that Prof. Anthony had a reputation as an electrician and as a physicist long before the American Institute was known. He built the first Gramme machine in this country, a historical incident that gives him of itself some notoriety and not a little fame. Prof. Anthony also sat by us in the years when the Institute needed the encourage-

ment of men such as he. You doubtless know that in its early days the American Institute of Electrical Engineers was regarded by scientific men more as a trade organization than as a scientific body. It was regarded in a somewhat hostile spirit by the persons who prefer to look at electrical science from the pure scientific and philosophical, not to say the metaphysical standpoint. Hence it must be to all of us a great satisfaction to know that at some of the first, if not the first meetings of any importance, we were honored by a paper read by Prof. Anthony before this Institute. I for one feel that Prof. Anthony did the Institute great honor at that time, when certainly it was not any brilliant honor to him, and the papers which he has presented at various times before the Institute subsequently, tend to show the interest which he manifested in the welfare of the Institute.

The token which our appreciative and perhaps over-modest fellow-member has provided has taken the form of a portrait of Prof. Anthony, which is to be presented to the Institute as a commemoration of the services rendered to us and to put on record before us in tangible form the remembrance of a most worthy presiding officer, whose zealous and untiring devotion to the interests of the Institute ever since its foundation, entitles him not only to remain in our sight, but in the hearts of all who have appreciation and gratitude for the great good, which Prof. Anthony has done for the Institute.

I will now ask the gentleman to unveil the picture. [Applause.]

THE CHAIRMAN:—I am sure it affords the Chair much pleasure to accept on behalf of the Institute this picture of our Past-President, Prof. Anthony. There is one point which Mr. Mailloux, in the course of his remarks upon Prof. Anthony, did not touch, and it is one I have heard his name connected with, and I think it is due to the gentleman to refer to it, and that is in connection with his interest, a pioneer interest, in the establishment of electro-technical education, or the technical course in our public institutions. The Chair also trusts that this picture which adorns our wall, is but the forerunner of others which we shall have the pleasure of looking up to. There are Past-Presidents who have also honored this association, and he does not doubt that he voices the feelings of every one of the members in saying that before a long time is past he trusts that the Institute of Electrical Engineers will have upon its walls the portraits of each of these Past-Presidents of the society. [Applause.]

PROF. ANTHONY:—May I be allowed to say one word, Mr. President? I was greatly taken by surprise when Mr. Mailloux began his speech. I had no idea of what was coming and it seemed to me that if the Institute or any particular member who wished his name kept secret, was going to present to me some testimonial on account of my coming down here and attending the meetings of the Institute, which it was a pleasure for me to

do, it was something entirely undeserved, and I am very glad that the matter has taken the form that it did take finally—that our friend has presented to the Institute a portrait, for the benefit of the Institute and not for me. I certainly feel very much flattered by the action taken, and I am very glad it was not a testimonial to me. [Applause.]

THE CHAIRMAN:—If there are no further remarks, we will next proceed to the paper by Mr. Carl Hering—"A New Graphical Method of Calculating Leads for Wiring."

Mr. Hering read the following paper:

A paper read at the General Meeting of the American Institute of Electrical Engineers, New York, May 20th, 1891, Vice-President Hammer in the chair.

A NEW GRAPHICAL METHOD OF CALCULATING LEADS FOR WIRING.

BY CARL HERING.

The subject of the calculation of leads for wiring has been written about in periodicals and books so often that it may almost be necessary to preface this paper with an excuse for again bringing up at this meeting such a simple and threadbare subject. The calculation of a single pair of leads is such a simple matter that very little need or can be said about it; the formula, if not at hand, can be deduced about as quickly as it could be looked up. But simple as this calculation is, if it has to be made not only tens, but hundreds of times, a method which shortens this work may perhaps be of interest to those whose misfortune it is to have much of this uninteresting work to do. The following suggestions of a graphical method is an attempt to reduce this work to what might be called "machine methods," by substituting for the usual figuring, a simple graphical device which requires no figuring at all and which is at the same time universal in its application, as it is adapted equally well for any of the many different lamps in the market to-day.

Makers and users of one particular lamp can readily calculate a wiring table for that lamp, for each of a few of the usual losses in the leads, and such tables are often quite convenient for those particular conditions; but it is evident that they are limited to those fixed conditions. Their scope can be increased by making simple reductions, but they may then become dangerous tools in the hands of many mechanics who may readily fall into such errors as multiplying instead of dividing by two. Furthermore, such tables are not generally applicable to branched leads, which are

so common in wiring, and mechanics (they cannot be called electricians) have frequently made the error of determining from the tables each successive section of a branched wire for the total loss. It is no wonder, then, that they can underbid their competitors. To make a set of tables large enough to cover such fractional losses means to multiply their number considerably. If in addition to this, such a set of tables is to include all the many different lamps of different makes and candle powers, it is readily seen that the number of tables which become necessary would be greatly burdened by the searches for the special table required for each calculation. As long as there are lamps of such different amperage and voltage, it would hardly be practicable to make a universal set of tables, especially when the currents which the lamps take are greater than what is claimed for them.

Graphical methods, therefore, suggest themselves, for in these the size of the diagrams does not increase nearly as rapidly for an increased scope as the number of tables would. Graphical methods have been devised to replace the tables, but they have not been satisfactory for various reasons. As early as 1884 a diagram was devised by Emil Waldstrom, which was universal in its scope and exceedingly ingenious. It was based on logarithmic functions and gave not only the cross-section, but also the diameter of the wire, for any lamps, distances or losses. But the dense network of lines, and the method of using it, were so complicated that only technically trained engineers would be likely to apply it in practice. In the same year the writer devised and used a diagram totally different in principle, which had the advantage over the other in being so simple that it could readily be understood; in fact, it could not be misunderstood, as there was only one set of diagonal lines. It consisted merely of a right angle and a series of diagonal lines drawn through the centre, the background being cross-hatched. It was not universal, however, as it had to be made for each special lamp; but it had the redeeming feature that any one could readily make such a diagram on cross-section paper for any particular lamp and scope. This diagram was described by the writer in the *Electrical Engineer* of January, 1887, and is doubtless known to many, as it has since been copied and published by some one else in a well-known book on wiring, though without any acknowledgement of the true authorship. This diagram, however, is only a partial solution of the problem, as it performs only two of the three multiplications and

divisions required by the formula; it is necessary first to multiply arithmetically the number of lamps by the distance in feet, the diagram will then perform the remaining multiplication and division, both in one operation, giving as the result the size of the wire or the gauge number. To attempt to give on a regular scale the diameter of the wire as distinguished from the cross-section, would require a series of parabolas to be drawn; this would render it practically out of the question for any one to make the diagram himself. The simple device was therefore resorted to, to use a regular arithmetic scale of cross-sections, instead of diameters or gauge numbers. All the lines then become straight diagonals and are therefore very easily drawn. Furthermore, the different factors which enter into the calculations were so placed on the diagram that the series of hyperbolas called for by the formula resolved themselves all into straight lines.

The objections to this method were that it required a different diagram for each different lamp, and that it did not multiply the number of lamps by the distance in feet. The object of the present diagram is to overcome these two objections. This was accomplished by the simple device of having three of these diagrams or sets of diagonals placed next to each other, as shown in the accompanying diagram, so that a line could be followed readily from one to the other, thus enabling a series of multiplications and divisions to be performed. The diagrams might have been superimposed on each other, but this would make a confusion of lines.

As this same principle of making a succession of calculations graphically may be applicable equally well to other formulæ besides that for wiring, it may not be out of place here to explain the principle before showing how this particular diagram is to be used. The general principle may be laid down that for any formula containing three variable quantities, one of which is the product or the quotient of the other two, the "curves" representing their relative values may always be represented by a series of straight diagonal lines drawn through the centre or zero point. Such a set of lines will therefore enable one to make any calculations graphically for that formula. For instance, horse power is equal to volts multiplied by amperes; the constant 746 does not concern us at present. A series of diagonal lines properly spaced will therefore give directly either the horse power, the volts or the amperes, when the other two are given. The curves

may also be made hyperbolas, but it is evidently absurd to draw hyperbolas when straight lines answer just as well and are even more accurate, as they can be laid off and drawn with great precision. Nothwithstanding this, such diagrams are often published as hyperbolas. The difference lies in the disposition of the scales. One scale is vertical, the other horizontal, and the diagonal lines (or the hyperbolas) each represent one unit (or number of units of the third scale. To make the "curves" straight lines, the diagonals must be made to represent one of the two quantities which is equal to the quotient of the other two, and not the one which is equal to the product of the other two, because the curves would then be hyperbolas. In the example given the diagonals must represent volts or amperes, but not horse powers. The constants in such formulæ affect only the positions of the diagonals; although they increase considerably the work of arithmetically calculating the results, they do not affect in the least the graphical calculations after the diagrams are once drawn. For this reason alone the graphical calculations are that much simpler.

Now, the general formula for wiring is as follows:

$$\text{Cross-section} = \frac{\text{current for one lamp} \times \text{number of lamps} \times \text{distance} \times \text{constant}}{\text{loss in volts}}$$

containing six quantities only, one of which is always constant, being equal to twice the mil foot resistance of copper, if the cross-section is in circular mils. As we have just seen, calculations involving three of these five quantities may readily be made graphically by means of a single set of diagonal lines. For instance, for any one make of lamp the current per lamp is a constant, and may therefore be combined with the other constant as one; if, furthermore, the number of lamps and distance be multiplied together mentally and then used as one quantity, lamp feet, there are then only three variable quantities to the formula, and calculations can then be made by one set of diagonals. This will give the diagram mentioned in the early part of this paper. But a universal diagram must be equally well applicable to any lamp; and as graphical calculations are so much simpler, more rapid, and less liable to errors than arithmetical ones, it is preferable to multiply the lamps by the feet graphically also. Both of these may be accomplished by splitting up this formula into smaller ones, each of which contains no more than three variable quantities. Each formula can then be calculated separately by a simple diagram, as described, thus permitting the whole formula to be calculated graphically.

To do this, let the first diagram perform the calculation,

$$x = \frac{\text{current for one lamp}}{\text{loss in volts.}}$$

In which x is a mere auxiliary quantity. Let a second similar diagram perform the next calculation :

$$y = x \times \text{number of lamps};$$

and a third diagram the final calculation:

$$\text{Cross-section} = y \times \text{distance.}$$

The constant may be combined with any one of these, it is immaterial which one. This triple calculation may at first seem to complicate matters on account of the new quantities, x and y. These, however, are easily eliminated by the simple device of placing the three diagrams together, side by side, in such a position that the two x scales coincide, and similarly the two y scales. By doing this, one has merely to pass directly from one set of diagonals to the next to perform the successive steps of the calculation, without being concerned about the intermediate auxiliary quantities. These intermediate quantities correspond, and are equal to the successive products or quotients which are obtained in the successive arithmetical multiplications and divisions of these five quantities in the formula, which cannot, of course, be eliminated in making the calculations arithmetically, showing another great advantage of the graphical methods. This particular method of performing the successive calculations is, of course, not limited to this formula, which is used here merely as an illustration. Any other formula which may be split up into smaller ones, each involving one multiplication or division, can be calculated graphically by the same method.

In the accompanying diagram the formula just given is represented graphically as described. It will be seen to consist of three right angles each with its set of diagonals and a common center. Each two right angles have one side in common which represent the quantities x and y, and which thereby drop out of consideration. The lower left-hand diagonals represent *loss in volts*, the upper left hand, *number of lamps*, and the upper right hand, *distances*. The remaining two of the five variables are represented by the starting scale vertically below the center, which represents the *amperage of a single lamp*, and the last scale, to the right of the center, which represents the final result, namely, the *cross section*. The constant is included in any one of these three sets.

To perform the graphical calculations, simply follow from one scale to the next on the cross-lined background up to the last scale, the cross-sections, which is the final result sought for. An example is worked out and illustrated by the broken line on the diagram. Starting first with the amperage of the particular lamp, .775, on the first scale, follow to the left (see the broken line) to its intersection with the required volt diagonal, 1.0 volt; thence up to the next diagonals, the lamps, 10 lamps; thence to the right to the feet diagonals, 50 feet; thence down to the scale of cross-sections, where the reading is about 8200 circular mils, or about a No. 11 B. and S. wire; the whole operation requiring about as much time as it would take to start the arithmetical calculations.

The relative disposition of the scales is also of importance in such a diagram. For instance, the amperage of the lamp is generally the same for a great many computations; it has therefore been made the first scale, so that it can be eliminated from all those computations by drawing a horizontal line, say in red ink, through this value and then starting all the calculations from this line, thus saving that first part of the work. Next to the amperage of the lamp, the loss in volts is often the same for many calculations; the volt scale has therefore been made the second one, so that it also may be cut out of these calculations. To eliminate this part, draw a vertical red line through the intersection of the ampere and the volt lines. All such calculations are then started directly from the intersection of this line with the lamp scales. Finally, by the dotted lines drawn through the last scale, as shown representing the cross-sections of the standard wire gauge sizes, the last part of the work is also saved. The only operation left to be performed, therefore, is to follow a horizontal line from the number of lamps to the number of feet; it would hardly be possible to have it simpler. As the majority of calculations are made for the same amperage and loss, this represents the most frequent conditions under which the diagrams would be used.

The range of such a diagram can evidently be made very great by having several sets of numbers on the scales, each set being say ten times larger or smaller than the other, the position of the diagonals will remain the same. By an accidental coincidence the position of the dotted lines representing the B. and S. gauge numbers, will also remain very nearly the same for different scales

of circular mils, even though they represent different gauge numbers. Furthermore, lamps and feet are interchangeable on the diagram. Although the same diagram may have several different sets of numbers, it is probably preferable to have a separate diagram for each set. A set of three diagrams, of which the accompanying is the first one, will probably cover the usual ranges. For motor currents the same diagrams may be used, by assuming that the current represents that number of one-ampere lamps.

The whole diagram might have been made much smaller by superimposing the three parts over each other, instead of placing them next to each other, the principle remaining the same. The three centres of the diagonals would then be at the three corners of a rectangle. It was thought, however, that this might cause confusion, owing to the great multiplicity of crossing lines, and, therefore, be a disadvantage rather than an advantage.

There exists among many persons, unfortunately for themselves, a prejudice against any graphical methods of calculations. They appear to think that because such methods do not come within the narrow scope of their little multiplication tables, they must be something very difficult and complicated which would take too much time and trouble to master; and that such methods are suited only to professors and college men. In point of fact, graphical calculations are much simpler than ordinary multiplication, and very much simpler than division. Graphical methods share this unwarranted prejudice with other short and simple methods of calculations, such as algebra, the slide rule, logarithms, etc. All such short methods carry their reward with them to such persons, and only to such, who take the small amount of trouble to master them. No one having many and complicated calculations to make, will ever regret it, if he spends a little time in becoming familiar with any or all of these short methods of calculating; it is his own loss if he does not take advantage of their existence.

Discussion.[1]

Dr. F. A. C. Perrine:—Some five years ago I had occasion to make a diagram of a similar type to that of Mr. Hering's. While not quite so universal, I think it might be interesting to the association. (Making a sketch.) I number these horizontal lines which represent ten divisions in each, by the volts and the lines at right angles by amperes. I use 10 as being the most conveni-

1. By Messrs. Perrine, Hammer and Mailloux.

ent. Then any diagonal drawn will be a line of constant resistance. If I also number the horizontal lines as distances, I use as convenient there 200 feet. Then, for calculating any size wire for a given distance—given loss, given amperage—I follow my line of amperes up until I come to the drop, and then follow the diagonal along to the line of proper distance in feet, and these vertical lines will correspond to the areas of the wire accordingly. Each division will correspond approximately to 20,000 circular mils and the whole method of using the table is simply to draw in a number of diagonals. If you are working at a constant, say five volts loss, then you draw in your five volt diagonals and you simply have your table. You take the amperage—say 20 amperes at 5 volts drop—that gives you that diagonal. For 800 feet you pass down the diagonal to this point, and drop down here, and this will give you the size of the wire in circular mils. Now, as all the wire manufacturers publish tables of circular milages as well as diameters, it gives you the size of wires practically, and taking 20,000 circular mils as being the distance of one of these spaces, it will give the small tenths equal to 2,000 circular mils, which is equal to about a No. 18 wire. It is, of course, exactly the same formula that Mr. Hering has used, with the exception that I have taken as my diagonals, lines of constant resistance rather than constant voltages of the lamps. As I say, I first started at this table about five years ago and it was only last week that I succeed d in getting these numbers placed to my satisfaction. I had these two lines reversed for the past four years and it brings the area of all the wires that are ever used in about that space, but if you simply turn them round to bring the volts and distances on the same side, it enables you to use a 20 division cross-section paper up to a 400,000 circular mil wire, and if that is not large enough you can simply calculate your wire for half the distance and multiply by two. Then on the line beneath this I have also calculated the carrying capacities, so that once finding the wire of the proper size, you look under it and see if it has the proper carrying capacity. [Applause.]

THE CHAIRMAN:—The Chair would like to state that Mr. Hering alluded to the fact that some time ago he published a table which has since been copied without giving credit to its author. I think that is a thing that has been very frequently done in electrical matters—in fact, much too frequently, and Mr. Hering's work in this line is one that has attracted universal attention, and certainly there has been no one who has given it more close attention. His reference to the diagram by Mr. Walstrom, which was made in 1884—from a historical point of view—it calls to my mind a diagram that was made the year previous to that by Mr. Fritsche of Berlin, the inventor of the Fritsche dynamo. It was not universal in its character, but in a couple of lines I can give you an idea of what it consisted of. I was during the years 1883 and 1884 connected with the Technical Department of the Ger-

70

feet

30

10

10

10

10

10

10

10

0

0

0

0

0

man Edison Company, with which Mr. Fritsche was connected as one of the engineers, and he took some tables which had been calculated for wiring leads and used only one type of incandescent lamp of one electro-motive force. His form of construction of this table, of which, by the way, I have one of the blue prints in my possession, which I will be very pleased to show to Mr. Hering, or others, was made in 1883. (Making sketch.) And the table was, of course, very similar in its character. You take an Edison incandescent lamp of 110 volts, and by taking this table and running the fingers up where these two lines meet, it would give us the circular mils of the wire for carrying a certain number of lamps a given distance. I think that was the first diagram of this nature that was made, and I take great pleasure in placing the credit for originating that in Mr. Fritsche's hands.

MR. MAILLOUX:—I think that all of these methods really depend upon the same principle, which is the general principle of the curve stated in Mr. Hering's paper. The curve described by Dr. Perrine can be assimilated to the same thing. The only difference comes, as I understand, in the choice of the variable quantities which are ascribed to the different lines, and in his case he superposes two of the diagrams. If he had superposed a third, it seems to me that he would have arrived at the same result that Mr. Hering arrives at by putting the three systems of diagrams together. This method that you have just described, it seems to me, is only one of the same things, excepting that the circular mils in that case are represented by the diagonal lines, which is only another illustration of how the different lines may be used to represent at will different factors.

MR. HERING:—I am very glad to hear of this diagram of Mr. Fritsche. An objection to it is that you have to follow two lines to their intersection. If you have a great many calculations to make, this is not as simple as following one line around a corner as in my diagram. The units in Mr. Fritsche's diagram could be changed about so that instead of following two lines you need follow only one.

THE CHAIRMAN:—I might add that Mr. Fritsche had at periods placed dotted lines upon his chart, which assisted the eye in following, and for rough work they answer all purposes very nicely, and by following out the line the calculation could be arrived at instantly by placing the finger at the intersection of the dotted lines If there is no further discussion on Mr. Hering's paper, we will proceed to a paper which is not down under the afternoon session. It is "A Thermo-Electric Method of Studying Cylinder Condensation in Steam Engine Cylinders," and I take pleasure in introducing to the society the author, Prof. Edwin H. Hall of Harvard University.

A paper read at the General Meeting of the American Institute of Electrical Engineers, New York, May 20th, 1891, Vice-Pres. W. J. Hammer in the Chair.

A THERMO-ELECTRIC METHOD OF STUDYING CYLINDER CONDENSATION IN STEAM ENGINE CYLINDERS.

BY EDWIN H. HALL.

The subject of cylinder condensation is one that possibly would come more aptly before the mechanical engineers than before the electrical engineers, and yet most electrical engineers are to a greater or less extent mechanical engineers, and the time seems not yet to be near when we can dispense with the steam engine in the practical application of electricity. The methods for turning the energy of coal directly into electric currents, I believe, have not yet been put into application. The subject of cylinder condensation is one that has always been prominent in the study of the steam engine. Before the time of James Watt, as you are all aware, at every stroke the cylinder was filled with steam and then it was cooled down by pouring water on the outside or by admitting water into the inside, so that whenever the steam was admitted it found the cylinder cool. A large part of the steam admitted at every stroke was turned into water at once on admission, and some of it was doubtless turned back into steam during the stroke, but much of it went out as water in the end. Watt introduced the independent condenser by means of which the steam is removed from the cylinder without cooling the latter down as it was cooled before; and yet, it is still believed that a very considerable part of all the steam that enters the cylinder is condensed upon the inner surface of the cylinder; that some of this is re-evaporated during the stroke, but that a very considerable part remains as liquid at the end of the forward stroke and is only turned back into steam during the back stroke, when it is a

disadvantage rather than an advantage, for it has to be expelled by the returning piston. It is considered that in some engines as much as 25 per cent. of all the steam that enters the cylinder goes through the cylinder as water—that is. during the forward part of the stroke. Writers upon steam engineering have devoted a good deal of attention to the discussion of the cause of cylinder condensation, with perhaps less attention to suggestions for remedy. The cause of cylinder condensation is this, that when the cylinder is thrown into communication with the condenser, rapid evaporation takes place of the water remaining on the wall, and that rapid evaporation under the diminished pressure cools greatly the cylinder wall. A little more than two years ago (Jan. 17, 1889), Mr. Dickerson of New York, gave an address before the Electric Club of New York, I think, in which he advanced the proposition that the peculiar character of the indicator card of a steam engine, which was supposed to show cylinder condensation and

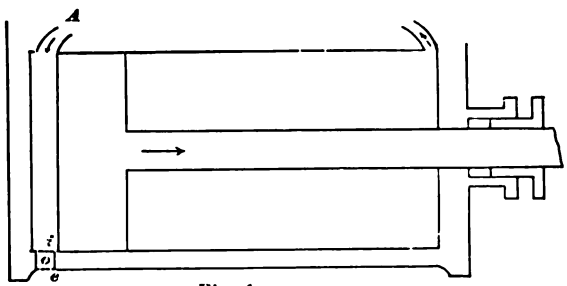

Fig. 1.

re-evaporation, was due to leakage by the valves—leakage in by the valves at one part of the stroke, leakage out by the valves at another part of the stroke. He made the statement that the steamers in the waters about New York City would travel four or five miles an hour with the valve between the boiler and the cylinder closed. I do not know how accurate that statement was. I have never seen any statement in contradiction of it. Mr. Dickerson's paper excited my interest—perhaps more than it would have excited my interest if I had been a trained steam engineer. I knew that this was a matter in controversy between steam engineers; that they were by no means at one as to the amount of cylinder condensation, and by no means in agreement as to what caused the cylinder condensation, some believing that the steam coming in here at A (Fig. 1) was cooled by contact with a layer of water remaining over from the previous stroke—the layer of water upon

the inside of the cylinder. They thought that you could hardly account by the action of the iron alone for the very sudden condensation of the large amount of steam that is condensed. Mr Dickerson's paper, then, stimulated me to undertake an investigation of this matter in a direction in which it had not been approached before, so far as I know Steam engineers have invariably, so far as I know, examined this question by study of the indicator card, ex-

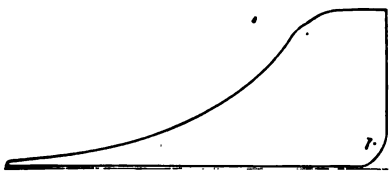

Fig. 2.

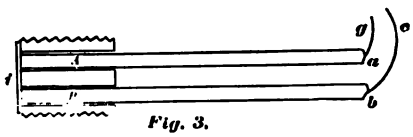

Fig. 3.

amining the pressure at different parts of the stroke—the pressure and volume—and finding out how much steam they indicate. Knowing the pressure at this part of the stroke, c (Fig. 2), and the volume occupied by the steam, we can tell what weight of steam there is in the cylinder as *steam*. It is found frequently that there is more steam in the cylinder at the end of the stroke than at the beginning of the expansion. According to Mr. Dickerson, that was due to the leakage by the valves by which steam was admitted, when the pressure in the cylinder fell low. It seemed to me that we might test the question how much heat enters the cylinder wall by a thermo-electric method. Let this line, *i e*, [Fig. 1] represent the thickness of the cylinder wall. Now this, o, is a hole which in the engine which I have used is bored for attaching the indicator. It is a hole about three-quarters of an inch in diameter. The thickness of

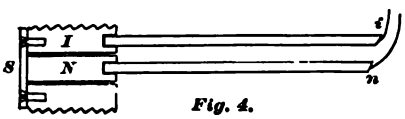

Fig. 4.

the wall is about three-quarters of an inch—a little more than that. I screwed into that hole a plug [Fig. 3] having at one end, *i*, a slice of iron—I had steel at first—of known thickness. Here is a hole going through the plug to that slice. Here is another. Into that hole I put a bar of antimony, A; into this hole a bar of bismuth, B The two were insulated from the plug. It is known to all of you that bismuth and antimony make an excellent

thermo-electric pair—the best that is in practical use. If I connect the antimony with the copper wire, *g*; the bismuth with the copper wire, *c*, and lead those wires to a galvanometer, I shall get a current through that galvanometer whenever the two junctions here, *a* and *b*, are at a different temperature from the section where the antimony and the bismuth strike this slice of iron. If I put the ends of these rods of bismuth and antimony into a pot of paraffin and heat that paraffin, when I get the temperature of these two junctions, *a* and *b*, where the copper joins the bismuth and antimony, the same as the temperature where the antimony and bismuth strike the iron, the current will cease. If the temperature here is below the temperature there, I get the current in one direction. If the temperature here is higher than the temperature there, I get a current in the opposite direction.

That was the beginning of the method. But it presently occurred to me that it would not do to use bismuth and antimony, for the reason that the heat conductivity of those substances is too different from that of iron. My purpose was to find what the temperature was at a certain thickness in the cylinder wall at a certain part of the stroke. (I close this circuit, *g c*, at a particular part of the stroke by means of a cam upon the crank. I can close the circuit at any part of the stroke for a short time.[1])

It being my purpose, I say, to find what the temperature is at a particular depth in the iron, I wished to have that temperature as exactly as possible what it would be if this iron were in a normal condition—that is, if this were in a solid cylinder instead of being in a prepared plug. To get that I found it necessary to have for these bars, A and B [Fig. 3,] a heat conductivity which was more nearly like that of iron than the heat conductivity of antimony and bismuth is. So I cast about for some metal which would *with iron itself* make a good thermo-electric couple, and of which the heat conductivity would be very like that of iron. Nickel suggested itself. Nickel and iron make a very good thermo-electric pair. I determined to make my plug in this way. Here [Fig. 4,] is a plug with a hole bored completely through it. Here is a slice of cast-iron, s, ground to a particular thickness,

1. In my latest experiments the fly-wheel has made about 60 revolutions per minute, and contact has lasted about one-sixtieth part of a revolution. The cam pushes two pieces of brass together without touching either of them directly. Springs hold the two slips of brass apart when the cam is not acting.

which is screwed on at the end of the plug, making a steam-tight joint—at least I meant it to be steam-tight.[2]

Into this hole I put a core of nickel insulated from the iron except here, where it strikes the slice. The nickel is soldered to the iron in this way. The slice is *tinned* very thinly, the end of the nickel core is also tinned. The two are heated very hot and then they are held together in a vise until they are cooled, so that the thickness of the solder there, is not more than a thousandth of an inch. The soldering was done with the slice and the nickel core in place, the slice being screwed to the plug and the core being surrounded with a single layer of paper to keep it free from the iron, except at the bottom of the hole. Slice and core were removed from the plug after soldering, and the solder which had been squeezed out from the joint by the action of the vise was carefully turned off in a lathe. The distance from the outer end of the core to the farther surface of the slice was measured carefully before soldering and after soldering. In no case did these measurements show the thickness of the solder to be as much as .02 mm.

I know very definitely, then, the depth at which the contact takes place. Now as to the heat conductivity of nickel, I can find no data in the books. I believe that no information in regard to it turned up in my studies. But I knew from the relation of its electric conductivity to that of iron that the heat conductivity ought to be nearly like that of iron, for usually heat conductivity and electric conductivity go hand in hand. I had to have a bar of nickel made to make the determination. I had a bar of nickel made about a yard long and about one square inch in cross-section, and I had considerable trouble to get it. I determined the heat conductivity of that, by comparison with a similar bar of cast-iron I found that the heat conductivity of nickel is rather less than that of cast-iron, perhaps 15 or 20 per cent. less[3], but still, nearer the heat conductivity of cast-iron than

2. In some of the experiments this joint was not perfectly tight, for water was sometimes seen to come out by the core, N, before the plug became hot. Generally upon such occasions the leaking appeared to cease when the plug became hot. The fact probably is that the joint continued to leak slightly but leaked dry steam, the temperature at the outer end of the plug being about 109 degrees C., and at the inner end considerably higher. Both experiment and reason indicate that the effect of such leakage upon the temperature found was slight.

3. The nickel used was of fair commercial quality. I am not yet prepared to publish details of the measurement of its conductivity.

any other metal that I could get that would make with iron a good thermo-electric junction. The specific heat of nickel is near that of iron, which again is as important, perhaps, as the matter of the thermal conductivity.

Now from the nickel core I led a nickel wire, both being made from the same bar of which I had determined the conductivity; from the iron an iron wire. This and the one of nickel were slender bars about 15 cm long rather than wires proper. [Fig. 4.] These are not drawn to scale. The junctions *i* and *n* were put in a pot of paraffin and I went through the processes which I have indicated already.

I had three plugs like this made. With one the slice of iron was half a millimeter thick (.051 cm.) with the other, a millimeter thick (0.101 cm.), with the other two millimeters thick (0.203 cm.). Let this [Fig 2] be the indicator card from the engine with which we are making this test. I used first the plug with the thinnest slice, and I found the temperature at this depth of iron, half a millimeter, at that part of the stroke, *k* [Fig. 2,] just before steam is admitted. I did the same with the millimeter slice and with the two millimeter slice. Let this line D [Fig. 5] represent 4 mm. of the thickness of the cylinder-wall, points at depths of ½ mm., 1 mm. and 2 mm, being marked upon it. Now I plot a curve. (Let me say that a part of this is prophetic; this curve I haven't been able to plot perfectly.) Suppose that distances out here to the left from the line D represent excesses above 109 degrees—109 degrees centigrade being the temperature of the outer surface of the cylinder wall. With the half millimeter slice at this part of the stroke, *k* [Fig. 2,] I find an excess above 109 degrees of 8 or 9 which I will represent by the distance *a*. With the millimeter slice I find the temperature to be greater at this part of the stroke, and I will represent the excess above 109 degrees by the distance *b*. With the two millimeter slice the temperature was not very different from what it was with the one millimeter. Through the tops of the lines *a*, *b* and *c* I draw a curve. (What I wish to do some time is to be able to plot a curve like that giving the temperature all the way down here inside the wall of the cylinder at this particular part of the stroke.)

I made similar determinations for that part of the stroke, *c* [Fig. 2.] I found the temperature at half a millimeter depth had risen to 131 degrees, more or less. The temperature at the depth

of a millimeter was a little higher. This point, one-half mm. deep, had already begun to cool. The fact is that in my engine the cut-off is like that [Fig. 2]—pretty gradual, so that re-evaporation very likely begins before cut-off is complete. I suspect, however, that 131° is too low for the temperature at a depth of one-half mm. At the depth of two millimeters the temperature was not as high as it was at a depth of one mm. At the depth of two millimeters in the iron there was, however, a jump of some 4 or 5 degrees centigrade.

One of these curves [Fig. 5] represents the condition of things

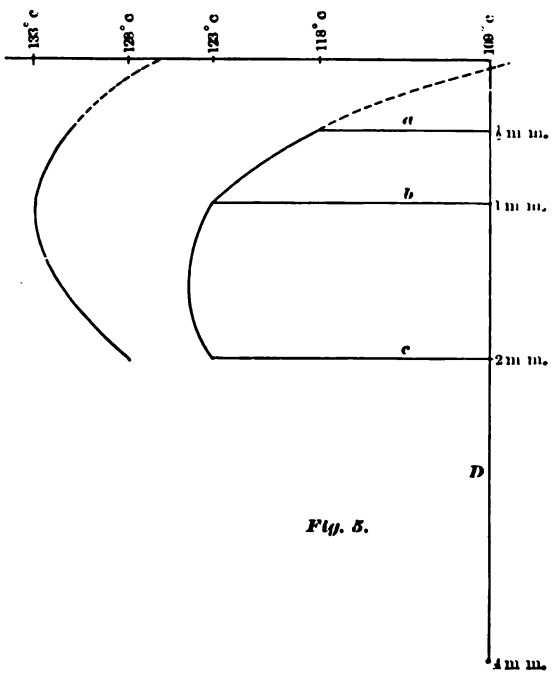

Fig. 5.

at this part of the stroke, *k*. The other curve represents the condition of things at that part of the stroke, *c*. The curves are by no means accurate and they are very incomplete, but supposing them made perfect, I have merely to put on here a planimeter to measure the area between those two curves, and it is then easy to determine how much the heat in the cylinder at this part, *c*, of the stroke exceeds the heat in the cylinder at that part, *k*, of the stroke, and this excess of heat is the heat which has been given up by the steam to the cylinder wall, which has been done during the admission. Allowance should be made, of course, for the

loss though the wall meanwhile. Then I can compare the result with the indicator card and see whether the two are in agreement.

I have made some tests for points at other parts of the stroke. I found, as I have already stated, that at a half millimeter depth the temperature at cut-off has already passed the maximum, that it falls rapidly down here during expansion, and at this part of the stroke near release, it is only a few degrees higher than at the end of the exhaust. But the parts deeper in the cylinder are, of course, slower to cool. In fact, the heat rushes into the wall here during admission; then as soon as the pressure is relieved the water begins to evaporate from the inner wall and the heat ebbs back and is spent, some part of it, in forming steam during the expansion, but not all of it. A considerable part remains at the end of the stroke to be spent in evaporating water which is still clinging to the wall of the cylinder.

As I have said, this research is by no means complete, but it has gone far enough to indicate that a very large quantity of heat goes into the cylinder wall. A very rough calculation from my experiments indicates that in the engine[4] I am using (Kendall and Robert's 10 inch cylinder with 15 inch stroke); about two-thirds as much steam is condensed upon the cylinder wall as remains active at the beginning of expansion. I should say that two-fifths[5] probably, of all the steam that enters that cylinder is condensed upon the cylinder wall upon the first quarter of the stroke. How much of that is re-evaporated during the stroke, and so helps more or less, I do not yet know. According to the indicator cards taken during the experiments with the plugs, the weight of steam in the cylinder at the end of expansion is about 1.25 times as great as just after cut-off. How much heat is absorbed by the layer of water which remains over from the previous stroke, I cannot tell. I think there is some evidence that a layer of water does remain over from the previous stroke. This examination shows, however, that the iron is exceedingly effective in cylinder condensation, and it suggests the question whether it is not possible to coat the inner wall of the engine with something which would prevent, to a considerable extent,

4. This maximum pressure I get here, Fig. 2, is about 33 pounds above atmospheric, and I use an expansion of about 3¼ and about 60 strokes to the minute.

5. This is very likely an overestimate; perhaps one-third would be safer

this condensation. It is not easy to find something with which one can coat the rubbing surfaces of the cylinder, I suppose, but there is the end of the cylinder and there is the face of the piston, and those two surfaces make, in the case of a short cut-off, perhaps more than half the area which is effective in this cylinder condensation. It doesn't seem to me at all impossible that some comparatively non-conducting material can be found with which those surfaces can be coated so as very materially to decrease the cylinder condensation. Experiments for that purpose could easily be made with plugs of this kind. Instead of having to coat the whole cylinder, one might lay on this plug some non-conducting material and see how much difference that would make in the amount of heat absorbed. I haven't attempted anything of the kind, but it doesn't seem fanciful to imagine that something valuable might come out of it, and a very large aggregate saving in the steam used in steam engines might perhaps be made in this way.

The research of which I have here given an account has been carried on with money from the Rumford Fund of the American Academy of Arts and Sciences.

For indispensable assistance in the investigation I am indebted to Messrs. Barron, Curtis, Hale, Kendrick and Page, members of a class at Harvard College engaged in a study of the steam engine.

DISCUSSION.[1]

THE CHAIRMAN [Vice-President Hammer]:—I think it is pretty generally admitted by engineers that even with the advanced knowledge of to-day, we do not know everything that goes on inside a steam engine cylinder, and these experiments of Prof. Hall seem to be highly original and are certainly very interesting. I hope that some of the gentlemen here who have had a large experience in steam engineering, will give us the benefit of their opinions upon these experiments of Prof. Hall.

DR. W. E GEYER:—I know of some experiments where an attempt was made to effect that saving of which Prof. Hall speaks, by using bismuth in parts of the cylinder. Bismuth is a metal having an extremely low conductivity of heat, and its specific heat is also extremely low. Under the circumstances, bismuth would not stand; it would gradually grind away or be torn to pieces and would clog all the apparatus.

PROF. NICHOLS:—I am not capable of discussing this paper from the standpoint of the steam engineer, but from the point of

1 By Messrs. Geyer, Nichols, Hammer and Grower.

view of applied physics it is an exceedingly interesting paper, both on account of the line of work and the method employed, and on account of the light which this method seems to throw upon the problem of heat in metallic walls. I would like to ask Professor Hall whether, in his opinion, something like lag is to be looked for in the performance of a thermopile on this method. Of course, this thermopile is subjected to rather rapid changes of temperature at its junction. To what extent must its sluggishness be taken into account in interpreting the indications of the galvanometer or does not that enter at all? I have often thought that this must be a factor which we should take into account in attempting to measure fleeting heat values at the junction of any thermopile. I have never looked into it with detail.

PROF. HALL:—As the thermopile is ordinarily constructed, there would be decidedly a lag. A thermopile is ordinarily constructed in this way: There is one bar, of antimony; here is the bismuth, and the two are soldered together for an indefinite distance. If you apply heat to the ends you do not know what you get—that is, if it is a fleeting thing, as you say. But you see what I have. I know what my junction is. There is a definite surface of contact, save for the very thin layer of solder already mentioned. When the heat strikes that I do not see how there can be any lag.

THE CHAIRMAN [Vice-President Hammer]:—Would any advantage come from the electrical welding of the contacts there?

PROF. HALL:—No, I do not think there would. I think electrical welding would make the thickness of the iron uncertain, somewhat. I suppose the pressure you would have to apply might change it somewhat. I have a plug which I propose to try, which shall not be soldered, the nickel core being simply held by pressure against the iron slice—to see what difference I shall get between this plug and the others. I haven't tried it.

MR. GEO. G. GROWER:—I would like to ask if it would not take an appreciable time for the heat to penetrate a depth of metal of half a millimeter, a millimeter or two millimeters. I should think there would be an appreciable interval of time of penetrating that depth of metal, so that instead of getting the temperature at the portion of the stroke expected, it might be at an advanced portion of it

PROF. HALL:—What I want to know is; not the temperature there at the inner surface, but the temperature here at my junction. The lag (in one sense) is the very thing I am investigating. I test that for different depths. If I know what the temperature is here at a certain depth at one part of the stroke, and what the temperature is at the same depth at another part of the stroke —if I can tell how much heat has gone into the iron between one part of the stroke and another part of the stroke, that is all I want.

Dr. Perrine then read the following paper:

A paper read at the General Meeting of the American Institute of Electrical Engineers, New York, May 20th, 1891. Vice-President Hammer in the Chair.

THE PRACTICAL ASPECTS OF ELECTRIC WELDING.

BY FREDERIC A. C. PERRINE.

It is hardly necessary to give any description of the general process of electric welding to the Institute of Electric Engineers, or to follow Mr. Lemp's paper of last year and our interesting visit to the factory of the welding company, with a detailed description of the machines. I intend rather to speak of them in their actual use, and to discuss some of the problems involved.

The plant with which I have been intimately connected, though not one of the heaviest, is still one making a great number of successful welds in a day, and is one in which the joints are required to be of the highest character and are subjected to the severest tests. The first machine actually leased by the welding company was taken by the Roeblings; but on account of the delay of the engine builders it was not in place and running for some six months—three or four months after other machines were in satisfactory operation. This was of the direct type, having a double winding on the armature, and the welding done on an apron immediately above the collector terminals of the heavy alternating coils. The pressure was applied by a handle regulated by the workman, the projection also being regulated by a scale stamped on the cam of the handle. Though considered crude, this machine did satisfactory work, often for twenty-four hours a day, and six days in the week, for about two years, having made about 370,000 splices in telegraph wire during its useful life.

After the automatic machines were brought to a reasonable perfection, we purchased a generator capable of welding up to ½ in. copper (40,000 watts), and installed one large trans-

former and seven smaller ones of capacity from No. 4 B. & S. copper to No. 18 B. & S. These have been at the works something less than one year, and show a daily record of over 1,000 welds in copper and iron wire.

Our attention was drawn to the process at first as a possible solution of a very troublesome problem in preparing and packing iron telegraph wire. For the convenience of the telegraph companies, this wire must be put up in bundles, measuring each exactly one-half mile of continuous length. It is very easy to say that the proper weight billets be bought and the pieces kept continuous through all the processes of manufacture, but as a fact these pieces are necessarily irregular in original size and are often broken in the process of manufacture and, in consequence, it has long been the custom to make the proper weight bundles by recoiling and splicing with the common telegraph or Lippman's joint, after the wire has been galvanized, jointing before galvanizing having proved a failure, on account of the acid carried over into the zinc pans by the great helix of the joints.

Singularly enough, the telegraph companies which had for so long accepted as satisfactory these cumbrous connections, immediately commenced to complain of the large burr left by the welding machine, and which we had thought unnecessary to remove. An emery wheel solved this difficulty, however, and we were then able to defy them to find the joint at all. After a very complete series of tests on the joints themselves, the telegraph companies have been finally satisfied and we are now able to make our bundles of the proper weight and length before the galvanizing is put on and thus save one rehandling of nearly our whole product, besides having removed one of the greatest difficulties and causes of complaint.

"Shorts," as they are technically termed, in copper, are of course a still more serious source of loss, not only on account of the greater value of the material, but also on account of the greater liability of this weaker metal to break in the process of drawing, and also on account of the necessarily defective character of a joint in the insulation with which copper wire is usually covered when sent out from the factory. Cables, as you all know, must be made in exact lengths, many of which are often very great, and though a twisted soldered joint might be satisfactory at the bottom of the sea, or covered by a lead sleeve in a manhole, it could not for an instant be tolerated in the body of a

cable which must, before completion of the cable, be passed through exact dies for covering with rubber or lead. Before the introduction of these machines, our custom had been to carefully scarf and fit the joint which was then brazed with silver solder. While this method makes a strong joint, but little increased in resistance and thoroughly satisfactory in other respects, it has the disadvantage of being rather slow and difficult to produce on account of the necessary care in fitting the scarfed ends as well as in soldering and finishing.

Since last November three boys on three machines have made respectively in this work 10,570 welds, 7,642 welds and 20,421 welds, the registers having been taken on April 16th. Every weld is tested by bending and straining, and if one shows any weakness it is quickly cut out and made again, with the loss only of a very small amount of time and the royalty to the welding company. In all of our welding of sizes below ¼ inch, the ends are prepared by cutting square across with a pair of pliers, and then setting the cuts at right angles to each other, touching only at the centre from which the heat radiates to the surface; then, after completion of the weld, the burr is removed by an emery wheel from the iron wire, and by a file from the copper. The preparation and finishing is a little more complex in the larger sizes, the ends are filed to a true butt and chamfered at the edges and after a reheating in separate clamps the burr of the weld is forged to the diameter of the original rod. The necessity of this will be apparent later on. All of this work is done on either soft steel or copper wire, and is probably of the simplest character done on any machines made by the welding company, consequently the advantages are not so apparent as where the welds are difficult in character on account of complicated shapes.

More varied work is done by the companies of whose plants I now give slight descriptions as likely to prove interesting.

Messrs. Seward & Son of New Haven, Conn., have at work a machine of somewhat universal character, its employment being principally in uniting Norway iron to Swedish steel in such shapes as are required in carriage irons and fifth wheels. In this work the burr is removed by a drop-hammer at the same heat by which the weld has been made.

The crescent tires of the hundreds of bicycles manufactured by the Pope Manufacturing Company, are welded and afterwards formed by dies at the same heat. The material is soft

steel. These people also have a machine for brazing their small parts.

Besides using the electric welding machine for their telegraph wire, the Trenton Iron Company of Trenton, N. J., have boldly attempted to make a weld in a wire rope to avoid the tedious and, with their locked wire rope, impossible operation of splicing. The ordinary joint in a wire rope is from ten to twenty feet in length, and requires considerable skill on the part of the splicer and his assistants. The operation is to re-twist bunches of wire from opposite strands, and to tuck the loose ends under the adjacent complete strands, which then hold them in place. With the locked wire rope, which is itself but a single strand, the only successful method of joining opposite ends has been to fasten securely around each a cast-iron collar, and after abutting the ends in a welding machine, to cement the whole together and afterwards to break off the cast-iron collar leaving the rope as a solid bar for a short distance at the weld.

Among the new solutions of old problems accomplished by the electric welding process, is that of the manufacture of spinning-rings by the Hopedale Machine Company of Hopedale, Mass. The desideratum of these rings is a uniformly hard surface, to produce which the method has been to stamp the ring from a sheet of metal and after finishing in a lathe, to caseharden the whole, involving, as one would naturally see, the loss of a considerable proportion of the original sheet. With a welding machine it is possible to form these little rings, about 2½ inches diameter, from a piece of bar iron, and after the burr has been reduced by a series of dies, to finish as before, with the result of a decreased cost and an equally satisfactory product.

One of the largest and most complete plants at present in operation is that of the Studebaker Bros. Manufacturing Company of South Bend, Ind. They have at present nine machines, one of them for steel axles up to 1½ inches square. After the weld is made it is quickly removed to a drop-hammer making about 300 blows a minute; then at the same heat the axle is set ready for the market. The test for these axles is to bend to 90° while hot, and then back to alignment. No instance has yet been reported of an axle breaking under this test, which is frequently made to insure perfect workmanship.

They have two machines for welding large car tires, about 4 inches wide and ¾ inch thick, and smaller sizes. The weld is

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made in the usual way and then quickly removed to a hammer where by vertical and side blows the burr is quickly removed. One hammer will easily take care of two welders in constant operation. They also have one machine for small carriage tires, 1 inch by ¼ inch, and smaller sizes. In this case the burr is removed by one blow of the hammer, after which the tire is set cold by a machine they call a cold stover, which embraces the tire on all sides and by great compression sets it up to the felloe more quickly and as effectually as can be done by shrinking in the old method. This, of course, is an immense strain on the weld, being a compression beyond the elastic limit of the metal. They have two machines for the hub bands which are used on the wagon wheel, and they are able to turn out about 1,200 on each machine. Besides these, there are two or three machines for smaller miscellaneous work. All of the metal used by them is either iron or low grade steel. After the introduction of their plant their machines made 360,000 welds inside of six months.

Probably the heaviest work done at present in electric welding, is that of the Johnson Company of Johnstown, Pa., who are not, as stated in the journals, welding long lengths of rails together, since with their continuous mill they already roll a rail too long to ship, but they find economy in the process in some details of their smaller manufactures. As to their results in general, they say: "We would state that over a year ago we put in one welder for general work. The bulk of the work done on this machine was work we had totally failed to do by hand welding, not on account of the difficulty of the weld, but on account of the general unreliability of the result. The result of a year's use of the electric welder was that we have not known one of the welds made by the method to fail, and we have closed contracts with the welding company for four 40,000 watt machines and two 80,000 watt machines, with the intention of adopting the method extensively in our works."

Agricultural machinery has generally been furnished with iron wheels, consisting of a malleable iron hub into which the spokes are upset, and of a rim into which they are upset at the opposite end. The electric welding machine has given us two new wheels, each of which are supposed to be preferable to the one described. In the first, manufactured by the Electric Wheel Company of Quincy, Ill., the hub is cast of malleable iron, with spokes about 3 inches long. To these are welded, by one machine, wrought-

iron spokes, and again by another machine these spokes are upset into the rim, which is itself welded into a tire, the product being a wheel of which the spokes are less liable to rattle loose at the hub. The second method is that of Niles and Scott, La Porte, Ind. The hub in this case is composed of two drop-forgings of low grade steel, grooved to receive the spokes. One-half is placed on the base of the welder and the spokes laid in their grooves; then the other half of the hub is put on and the clamp brought down. The current is then applied and the whole, spokes and hub, welded solidly.

The Rogers Typograph Company, Cleveland, Ohio, are practically welding brass to steel, making as high as 800 welds per day, and thus effecting a great saving in the manufacture of their type-setting and founding machines.

The hardest steel at present successfully worked by this process is the welding of band saws by the E. C. Atkins Company of Indianapolis, Ind. Besides the regular work of making the joint in continuous band saws, these people have ingeniously adopted this method for replacing broken teeth in finished saws. Formerly they were compelled to cut down to a smaller size any saws from which one or two teeth had been accidentally broken, thus losing not only the difference in price between the two saws but also the entire cost of labor in cutting the original saw. At present when a tooth is broken out, they fit in it a new tooth, which is electrically welded in place, and a drop of oil applied at the completing of the heat restores the temper to a serviceable point.

This, one can readily see, is of the greatest value in reducing one of the sources of imperfect work.

Pipe welding, which we all saw successfully accomplished at the factory last year, is put in practice by the Pennsylvania Railroad Company as well as by the Columbus Iron Works, Columbus, Ga.; Blymyer Ice Machine Company, Cincinnati, Ohio, and the Electric Pipe Bending Company of Harrison, N. J. This latter company is engaged principally in manufacturing long continuous coils for ice machines, blast furnace tuyeres and radiators. With this machinery they not only weld extra heavy black pipe, but also electrically heat the pipe with a machine 28 inches between the clamps for the purpose of bending to desired shapes over pipe formers.

With continuously spiraled coils of pipe it has been necessary

for a smith to weld the whole length, 150 to 200 feet, before attempting to bend any portion of it, and then after once the bending operation has commenced, in order to get a uniform spiral, the heat is not altered or the bending operation suspended till the coil is complete. Under the present electrical methods the coils are continuously heated in a fire, and the coiling is continuous while one length of pipe is welded at a time, the operation being to attach the welding machine to the end of the moving pipe, and while the forward portion is undergoing the process of bending, to weld on a new length, which will be accomplished in a sufficiently short time not to interfere with the coiling apparatus. In all of this pipe welding the burr is beaten down by means of a pneumatic hammer which is put in rapid motion by the act of closing it around the pipe, a rapid hammer having proved much more effectual than any press or dies. This is probably on account of the more rapid cooling action of the press, since no forming is attempted before the weld is completed and the current is turned off.

Three types of machines of importance are soon to be in use for the purpose of our army and navy, and the early recognition of the importance of this method of manufacture, speaks well for the ingenuity of our government designers.

In the new wire-wound guns, one of the most important specifications for the wire was that it should be readily jointed by the electric welding process, and in the Crozier gun now constructing the wire will be wound in one continuous piece under a constant and heavy strain. This wire is one-tenth inch square, with 180,000 lbs. breaking-strength, and 100,000 lbs. elastic limit, tinned as lightly as possible. The gun is to be made by winding the wire over a steel tube and shrinking a ring over the whole for the purpose of longitudinal strength. The welding process is simply the same as that described for round wire, and is reported as having given satisfactory results.

For the Charlestown Navy Yard, there is about completed a machine for welding ship chains up to 2 in. diameter links. These links are formed in halves and fitted roughly to each other, the welds being made in both sides at once by the same heat, which can be regulated to force either side at the will of the operator. When completed, both welds are forged immediately in a die by the same hammer. By this method only the points to be welded are heated, and as these are immediately forged one could expect

from the process a nearer approach to the strength of rolled metal than where the whole link is heated in the ordinary fire to a welding temperature.

The latest development for government work is the manufacture of conical shells and shrapnel for the smaller armor-piercing guns. With the modern type of armor, a cast shell is not of sufficient strength to resist the shock of impact and in consequence a chrome-steel point is welded to a tool steel body, embracing the powder cavity. This latter is but small in comparison with that in the old round shell of revolutionary days, consequently the accurate forging of the interior becomes correspondingly difficult.

Cast shells are made from the higher grades of crucible steel, but with these the casting is both difficult and uncertain, necessitating a large amount to be allowed for machining to a uniform wall in the probable case of a decentered core. The electrically welded shells are made entirely of forged point, body and base, which may be finished to approximate dimensions, these requiring after welding a minimum amount of machining. The tests on these shells have resulted in a contract for a large number under a guarantee of the manufacturing company.

This brief sketch of the representative uses of the process, I have given not only to show the practical applications of electric welding, which are undoubtedly interesting in themselves, but with this introduction I wish to call attention to some of the limitations and necessary precautions, as well as to the light which the results of this method of applying heat to the metals throws upon some of the obscure and disputed points of structure and constitution.

Welding in the ordinary sense, on an anvil by the means of a forge, is essentially the uniting of two surfaces of a metal which has previously been rendered plastic and which, under the hammer, can be made to intermingle into a structure similar or identical to the original metal; in consequence, this can only be accomplished with materials capable of passing into a plastic state before actual melting takes place. One of the most familiar examples of this weld to electrical men is the common splice in the gutta-percha core of a submarine cable. Provided only that the surfaces are clean, it is possible to make this material unite as perfectly as before it was cut. Amorphous waxes and similar substances can be treated by a like method; but when we reach

truly crystalline materials it is no longer possible to effect this union below the melting point, unless great pressures are applied. In passing from the one to the other condition, we see the phenomenon of the familiar cementing of ice under pressure great enough to produce what is called regelation.

Now, viewing metals in the light of these simple examples, we find that it has long been possible to weld amorphous iron under moderate heat, but for the highly crystalline steels and other metals a pressure high enough in ordinary practice has not been available.

Welding cold has been accomplished in a most striking manner by Professor Spring of Liège. Using small cylinders filled with powders of highly crystalline metals, he has not only succeeded in compressing them into compact bars, but has also succeeded in making alloys with all the characteristics of the same alloys produced in the usual manner in a crucible. These results go very far towards proving the absolute possibility of a weld in any material, however fibrous or crystalline. The electric welding process uses rather the method of the crucible than of the press, for although considerable pressures are applied during the operation, they are not beyond the limits of ordinary forging, and the essential difference is the greater heat available at the direct point where it is most needed, a heat which approaches and often reaches the melting point of the material. As the pressure is applied it is transmitted by the cold bar directly upon the softened metal and forces it to unite more readily. This view of the process brings us at once to some of the most important limitations; cast-iron, cast-steel, cast-copper, cast-brass are not changed in their essential constitution, but are united by a weld having all the characteristics of the original bar. Now, let us take a bar of rolled zinc or drawn copper and compare it with the cast metal. We find the crystals, formerly large and widely separated, especially in the case of zinc, where often internal oxide stains are perceptible, are now broken into minute fragments, elongated and united so closely as to allow the greatest opportunity for the action of cohesion. Weld such a metal as this, and the joint where broken will immediately show a rearrangement of the particles into larger and looser crystals, similar to those of the unworked metal. By repeated fracture of the bar you will be able to follow graduations of the action till it loses itself.

A weld of this character will give a sufficient tensile strength,

but the brittleness of a coarsely crystalline material has returned. Whether such a joint is worthless is entirely a matter of circumstances. With small sections the action is not perceptible on account of the very small length heated and also because bending, however short, does not subject the metal to so great a strain as in the case with larger sections. For a rod to be subjected to further drawing, rolling, or other like processes of manufacture, the weld is satisfactory, since the subsequent working reinstates the metal to its best condition, but for a wire to be considered a finished product further manipulation is necessary. In view of this, the later machines for welding heavy copper have been provided with reheating clamps and light rapid hammers, by means of which the burr, after a slight trimming, can be reheated and forged. In special cases a further hammering cold is added. Though this produces a great improvement in the metal, it yet leaves a good deal to be desired where great strength and pliability are required. To produce this a further upsetting of the metal throughout the whole heated space, and a subsequent forging will be necessary. This, up to the present time, has not been provided for and presents a greater difficulty than is at first apparent, besides furnishing a new proof of the crystalline constitution of copper. On account of this characteristic, as is apparent on attempting upsetting in the welding machine, it offers, at a red heat capable of moving and rearranging the particles, a resistance so high to compression that three or four times the pressure necessary for welding will not have the slightest effect towards increasing the diameter or upsetting. With bronzes the further difficulty is encountered of red shortness between the molten temperature of the weld and the malleable red temperature. That this should be so necessary and offer so many difficulties may seem strange, since the metal does not apparently exceed the temperature of ordinary annealing, and it must be that either the passage of the current by an extra disturbance helps to rearrange the particles, or else the temperature is greater than we imagine from an observation. The latter explanation seems to me to be the more likely, since our ordinary observations are made on metals heated from an external source and of which the surface is at the highest temperature. With the weld, however, the source of heat is internal and the surface is undergoing a continual cooling from the external air.

Welds made in the higher carbon steels are at a similar disad-

vantage and must be looked upon with disfavor unless provision for upsetting and forging hot is provided. These disadvantages are especially apparent with steel wires of the highest grade, where the strength of the original material has been more than doubled by tempering and drawing. In such cases it is not ordinarily the loss of tensile strength that is to be feared, but the increased liability to snap under repeated bendings. This is especially important in the case of wire to be twisted into a running rope such as is used on our cable railroads. From this view, the weld in a whole rope where forging is impossible, would be necessarily defective, and though for a standing rope I have no doubt it would be fairly successful, yet for a running rope I believe everything tends to prove it dangerous. It has been suggested as an explanation that these effects are due to the presence of oxygen at the weld, producing suboxide in copper and reducing the carbon in steel. In support of this view the case of the application of oil to the hard saw-teeth is cited as supplying the necessary carbon by its decomposition. It is undoubtedly a fact that by the heating of these high-grade steels and subsequent slow cooling some of the combined carbon is changed to the graphitic form, with a consequent loss of strength, and it seems clear to me that a drop of oil at the right moment serves to restore this or, in other words, effects a tempering. It is almost useless to attempt an explanation by a complicated chemical decomposition, when the simpler molecular rearrangement is more in accordance with the facts and a more complete explanation.

This brings us to a point where the electric welding throws light upon the actual production and nature of burnt steel. By forcing the heat in welding, no matter if rapidly, this effect can be produced as thoroughly as by the means of a fire, while if the name "burnt" was an accurate description and the iron and carbon oxidized, a sufficient time would be necessary for its penetration into the centre of a bar of metal. This agrees with and is a further confirmation of the most modern ideas about burnt metal, that it is a molecular rearrangement, and by careful forging and heating the metal can be restored to its original state. In this case oxidation certainly does not take place, though the action may be similar to metamorphism in rocks; steel being at the present day looked upon as a mineral, a matrix of iron including minerals which are generally compounds of iron with carbon, silicon, sulphur, manganese or phosphorus. Under this view

there may be internal chemical rearrangement at high temperature. With carbon we know this to be the case, but I take it as settled by electrical heating that the change is not necessarily an oxidation.

The welding of two different materials, such as iron and steel or malleable cast-iron, presents another difficulty and possible danger. When such a weld is broken we find that the pressure in the plastic state has forced the iron over the steel or malleable, which has itself been merely slightly melted and cemented to the adjacent particles of the iron. This produces what might be called a reverse riveting, the hot iron having been upset over the steel and inclosing it tightly, in the place of having been upset in a hole as in ordinary riveting. Again, as in all this work, the tensile test is but a poor test of quality, and though a weld may have within ten or twenty per cent. the tensile strength of one of the original materials, it is a fallacy to say that therefore the weld is within ten or twenty per cent. of the quality of either of them. Use here again determines the serviceableness of the process.

I am tempted here to carry further the discussion of the special cases offered, but it would only be to apply over again the observation indicated in the foregoing, and which might be finally stated that although the electric welding process will not practically splice everything from a bar of magnesium to a steamer shaft, yet it enables us to accomplish results otherwise impossible and if both the use of the product and the treatment of the materials be sufficiently studied, it may be trusted in its results.

Discussion.[1]

The Chairman (Vice-President Hammer):—We shall have great pleasure, I am sure, in listening to Prof. Thomson.

Prof. Thomson :—I would say, Mr. President and gentlemen, that my time has not been very closely given lately to the applications of welding processes in the arts. That department of the work has been very ably taken care of by Mr Lemp, whose paper the Institute heard last year. Of course, I have inspected the machinery as each type was brought out, and made suggestions constantly on this or that point, and may be considered, therefore, as a sort of privileged observer of results. There are gentlemen here also who may be able to give you more information in regard to the development of the business than I. I haven't paid

1. By Messrs. Thomson, Hammer, Maver and Lockwood.

attention to its details. I would say, however, in general, that the tendency in the development of electric welding seems to be to produce new processes or to give rise to new applications of mechanical principles, examples of which are seen in the wheels before us. There we have practically a development of a system of work depending entirely upon electric welding, and that seems to be the direction in which it is to grow most rapidly. Naturally, when it comes to supplanting ordinary processes of welding, there is that inevitable prejudice to overcome and at the same time the reluctance of the manufacturer to throw aside a plant which he already possesses and which does his work fairly —going to something entirely new—putting in an extensive electrical plant which he is afraid of, feeling that perhaps something may happen to it, and it may require repairs. He prefers that his neighbor shall get all that kind of experience before he takes hold himself. This is very natural, indeed, and it simply means that in certain directions the electric welding operations will probably be slower in development than they would otherwise be. But with the special cases, many of which you have had mentioned in the able paper presented, you can see for yourselves that there is a direct and easy growth.

In relation to the wheel before us, in which the cast hub is joined to spokes, there is one point that I may mention, that the possibilities are very well shown in that wheel, of performing a number of welds at one operation. There we have the spokes set in place in the machine and radiating towards the hub, and the machine grasping the spokes and welding them all at one application of current, so that we have a number of welds made in multiple, as it were. The other case is an excellent example of a different procedure, where the spokes radiate toward the centre and are caught and pinched between the two surfaces which are being welded, and are therefore securely fastened. These are some examples of processes capable of being performed in the new art.

In regard to the changes of structure which are undergone by materials subjected to the welding process, that was an effect noticed, of course, rather early. It was particularly prominent in the case of hardened steel, which underwent a decided change during the welding. Suppose the weld is made here (indicating). It would naturally be expected that whatever change was due to the application of heat above a certain temperature, would be found to have occurred in this metal at the weld. It would also be naturally expected that such action might proceed back from the weld a certain distance or, in other words, that if this bar had been tool steel, made under the hammer or in rolling, and had been thereby given a certain texture by the mechanical kneading or by the hammer, that a relaxation of such texture or a change of such texture might occur for a certain distance on each side of the weld. This is an actual fact. A tendency to

granulate exists there. The old idea was that the steel was burnt, but I have tried this operation in a hydrocarbon atmosphere and it occurs just as well in the hydrocarbon atmosphere, so that it is not a burning process. It is simply the relaxation of the molecules, the change of physical condition, or perhaps a different chemical combination brought about by the high temperature. To a certain extent this action occurs with other materials and, of course, with fibrous drawn copper it is present. Any metal whose properties depend upon the working, any metal a portion of the strength of which is due to having been drawn or hammered, of course undergoes that relaxation or rearrangement or tendency to granulation such as is found in a casting where the metal has undergone the melting process and has been allowed to cool afterwards. It is also a noticeable thing that we can take a bar of wrought-iron and put it in the welding clamps and pass a current through it, heating a portion, and so carry it to such a temperature beyond the welding temperature, as will actually change its nature. The bar can be made, by carrying it to a sufficiently high temperature, to become a spongy burnt mass—that is, the temperature will have been made so high that the iron, like the steel, undergoes a change of condition and does not re-

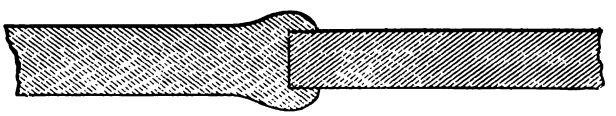

FIG. 1.

turn on cooling. I attribute this largely to the fact that in the ordinary operations of the working of the iron, the temperature has been below that point. The iron has been manufactured below that point in the furnace operations and that when we raise the temperature beyond the point of the making of the iron, we set free occluded gases which have been in the iron. We make the iron, as it were, puff out in a spongy form by setting free between the fibres the hydrogen gas, or at least in the substance, and consequently the iron breaks very easily there, and if examined it is full of minute cavities which show that occluded gases have been expelled.

Another interesting feature of the welding operation has been mentioned, which is that some materials when put together do not mutually yield during the heating; one, as it were, overlaps the other—the reverse riveting, as it has been termed by Dr. Perrine. One melts or softens and slips over the other, making a joint something like that [Fig. 1.] For many purposes, that is a very strong joint, particularly if the burr or expansion is not removed. There has been no change of form, or very little change of form of one of the bars. Even if one bar doesn't change at all, the union may still be a perfect one. It may be

that one piece, in fusing over the other piece, has brought about just the same intimate relation of molecules of one and the other bar, as though both had undergone a deformation or change. But in most cases it would naturally be expected that the best joint would be produced by, as it were, bringing to the front new molecules, perfectly clean and unchanged molecules, from the interior, in contact with the molecules of the other bar, which are also spreading and moving; and this is actually true. Advantage has been taken in a few instances, of the kind of action in which one bar only spreads, to make a joint something like this [Fig. 2.] Cut a number of steps in one metal and cause the other, as it were, to dovetail itself by a simple flowing over, and that in some instances makes an excellent joint. The metal flows over and fills the spaces all around, and if they have any tendency to unite, such as brass and iron have, or brass and steel, of course the union is very strong.

Another point in relation to the practical application of electric welding, and then I am through; and this is that it is curious to notice how prejudice sometimes stands in the way of the best results. There are undoubted cases where the burr or extension left by the welding operation would be best left on, because

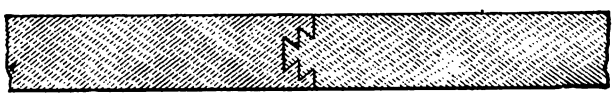

FIG. 2.

there is an increased section there and unless this section is to be very well and heavily forged, the strength through the weld will be greater and the bar will rarely if ever fail through the weld; but from the mere habit of not seeing things done in that way, where the expansion would not do any harm if allowed to remain, people want it removed. It could in many cases be forged up to form a handsome bead, but in most cases, on account of prejudice, it must look like the ordinary scarfed weld or it is not altogether marketable. That, I think is a temporary condition for many instances of welding. It will be gotten over finally. Dr. Perrine has brought out the fact in relation to the telegraph wire, that where formerly they were content to take the double-twisted joint, with all the metal which is involved, and when they got an electric weld to take the place of that they were still not content. Their self-induction was high and nothing would satisfy them but that the slight burr should be removed, which was of course a simple thing to do, either by forging it down or by grinding it off.

One further point in relation to the upsetting for copper, the structure of which has been changed. This [Fig. 3] figure I make might represent a copper joint made of rather large cop-

per. Now, naturally if that joint has been formed, the copper has been heated back here some distance at *a*, on account of its high conductivity for heat, and if that part of the metal has a structure due to mechanical working, that structure is partly lost

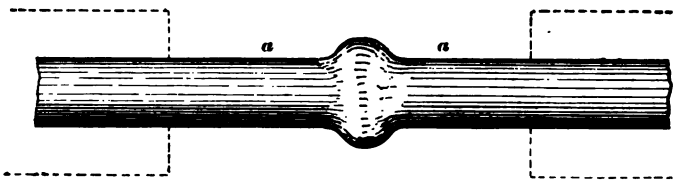

FIG. 3.

if not entirely lost at this point. It is desirable on account of the resistance to strains required for some puposes, that it should be restored. I found in the early days that I could, by taking the clamps apart a little, swell up a certain amount of material and then hammer the mass down to its original condition and get the effect of working back. But when this comes to be attempted on rather large stock, you can readily see that the power required is considerable. We must be able to take hold of that bar very firmly indeed, and we must have hydraulic pressure without any slipping, to force up a mass of material at a rather low temperature, because if you heat it to too high a temperature, the difficulty is that you do no not upset the bar far enough back. You require to take in almost the cold metal in the upsetting process and then forge it back to the original shape to get the fibrous structure back. That probably can be done by taking hold of a sufficiently great length of bar on each side of the joint and by putting guides on each side of the work, so that it will not buckle out of shape and so take hold, in other words, at a number of points by multiple clamps so as to force the metal up If we should use notched clamps, there is an objection at once, because we do not want an injury done to the exterior surfaces. We have to use a smooth clamp, a clamp which will not bite into the material and change its surface. We would probably have to take a foot or more of the bar, plant it firmly in clamps fitted closely to the work and then force it, risking the slip [Fig. 4]. Of course, we cannot get

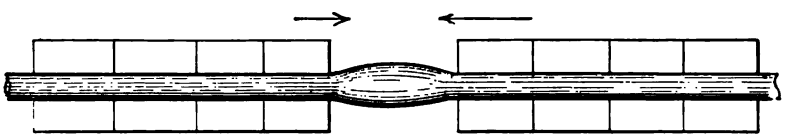

FIG 4.

an end pressure on a bar which may be very long—100 or 200 feet, or more than that—or even a very long coil, so that we have to get whatever hold we can by hydraulic pressure applied laterally, and risk any deformation that takes place. [Applause.]

THE CHAIRMAN:—The electrical engineers have had to meet from their brother engineers in the development of the various stages in the department of electricity, a good deal of opposition and cynicism. The gas engineers we have had to meet with the development of the electric light. In the transmission of power we have had to meet the mechanical and civil engineers. Those two departments of electricity have been pretty well established and our brother engineers have been converted. But to-day, to use an expression of Prof. Thomson, there is a considerable amount of self-induction present in the minds of civil and mechanical engineers on the subject of electric welding. We would like to have a good many of them here this afternoon and show them some of the practical evidences of the commercial state of the art as it is to-day. This is an important paper. It is one which should attract and will attract a good deal of attention. There are gentlemen here with us who can give us further information on this subject, who we should be very glad indeed to hear from.

MR. WM. MAVER, JR.:—With regard to the causes that led to the objections to the burr left by the electric weld in the case of the telegraph wire, and without knowing the exact cause in the case in point, I would suggest that it may have been due to the fact that the presence of the burr prevented the free working of the wire in the act of stringing on the poles. In the case of hard drawn copper wire, this was one reason that led the companies to forbid the use of any "splices" in the coil. Another reason was that factory "splices" were found to conduce to breaks in actual service. Still another objection to a certain form of sleeve joint was that it facilitated permanent crosses between wires that otherwise would only have been swinging crosses.

MR. LOCKWOOD:—In common with the remaining portion of the audience, I have been particularly interested in this subject. I have watched it with great interest from the moment it was put before the public and before the profession. But this is the first time I have ever had an opportunity of hearing a paper read upon it or of hearing any discussion. I have always been so unfortunate as either to be absent from the country or otherwise engaged when the matter has been under consideration. It often occurs, I find, that different paragraphs in a paper of this character will impress different minds in a different way, and the paragraph which made the most impression upon my mind was that in which the union of two pieces of gutta percha was referred to. I find that this paragraph reads:

"Welding, in the ordinary sense, on an anvil, by means of a forge, is essentially the uniting of two surfaces of a metal which has previously been rendered plastic and which, under the hammer, can be made to intermingle into a structure similar or identical to the original metal. In consequence this can only be accomplished with materials capable of passing into a plastic state

before actual melting takes place. One of the most familiar examples of this weld to electrical men, is the common splice in the gutta percha core of a submarine cable."

It goes on to say: "Provided only that the surfaces are clean, it is possible to make this material unite as perfectly as before it was cut. Amorphous waxes and similar substances can be treated by a like method, but when we reach truly crystalline materials it is no longer possible to effect this union below the melting point, unless great pressures are applied."

Well, Mr. Chairman, in one period of my checkered career I was engaged in making plate glass. It is rather difficult for me to decide under which of these classifications glass comes. Whether it is a material that passes into a plastic state before melting takes place or whether it is amorphous wax or similar substance or whether it is a truly crystalline material. I have seen it in conditions which approximated to either one of those conditions. It certainly does before it melts, become plastic. I would like to ask whether the gentleman who wrote this paper and who has read this paper, can tell us whether anything has been done in working glass by electricity and whether that has come within the realm of his operations, or whether it is likely to. It is very difficult, of course, to imagine how you can get glass to be acted on by electricity, but there are so many things done by electricity nowadays and so many more things claimed to be done that I should not be surprised if that comes in one or the other category.

THE CHAIRMAN:—Perhaps Prof. Thomson has had some experience in this direction.

PROF. THOMSON:—The only experiment that I can recall at this moment is this: A tube of glass slipped over a platinum wire with current, and the glass heated by the wire so that the glass came down and stuck to the wire. But in the ordinary operations of the work of incandescent lamps, glass is sometimes rendered plastic, as we are quite well aware, and sometimes the heat from the filament or the filament itself touching the side of the glass softens it and the glass bulges in and makes holes. That, I think, must be taken as an instance in which electricity has got something to do with *working glass the wrong way.*

MR. LOCKWOOD:—In New Albany, Ind., in the year 1869, while working at a window and plate glass works, I found that the only way known at that time practiced of cutting off the end of the cylinders which were first blown and which were to eventuate in window glass, was to take a portion of glass from the pot on the end of a blow-pipe or pincers and twist it around the end of the cylinder for a moment, and then take it off and drop a little water on the place where the melted glass had been, and the end of the cylinder would then crack off at once. The cylinders were then placed in the flattening oven. Before placing them in the oven though, they were run with a red hot bar lengthwise. Then

they were placed in the oven and when they became plastic were flattened. Prior to that time my electrical knowledge had been gleaned by telegraphing. Nearly every young man at that time, who was engaged in the telegraph business, had at least invented a button repeater, and when I left the business temporarily I did not give up my interest in electrical work. Thus while watching the men taking the ends off these cylinders the way they did, it struck me that a fine wire, if heated by the passing of an electrical current, might be used to take off the ends of the cylinders, and I invested all my spare cash in a battery, and I got enough current to make an iron wire red hot. It worked like a charm. The glass blowers at that moment looked upon me as being something of a medicine man. After doing it two or three times, however, I found that my means at that time would not enable me to keep it up, although the thing was done and done well. I speak of it at this moment simply to show that there was an application of electricity to the manipulation of glass, and I may further say that within the last decade some fellow has patented that process.

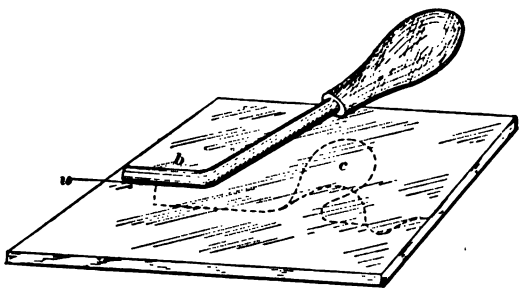

FIG. 5.

PROF. THOMSON :—Mr. Lockwood's remarks do recall another instance of a somewhat similar character. I can illustrate it better by going to the board. It depends on the fact of glass *not* being a material which can be made plastic at ordinary temperatures. Perhaps it is in relation to its being a brittle or crystalline material. You are all familiar, no doubt, with the fact that an iron bar can be used to cut glass, whether it be a cylinder or flat plate—that you can heat a piece of iron red hot and apply it to a glass plate in a proper way and lead a crack all over the glass as desired. Say this is a piece of window glass. You take an iron bar at a low red heat, having filed a notch, N, in the edge of the glass plate. [Fig. 5.] Now you take the iron and put it about ⅛ inch away from the notch N, resting it on the plate. It is generally best to have the iron bent up a little and the part B red hot. You put it near the notch red hot, say there. The crack starts for the iron and stops there. You keep moving the iron ahead and you can cut the glass

into any shape you want, leading the crack all around in curves or in any other way; but you must be careful of one thing—never to return on your tracks and try to cut as at c, or it will break the plate irregularly. Now, it occurred to me that this operation could be much more easily done electrically, and so I constructed an arrangement for heating a small short wire and I arranged a corresponding wire on the other side of the glass, on the principle that it is best to heat the glass on both sides and so obtain much greater certainty of the result, and I found that with such a tool I could cut very thick glass with perfect certainty by using the heated wires both on the under and upper sides of the glass. The glass would split and follow the tool, the crack was also started very readily. Another thing developed at that same time. I found that I could cut a beveled edge on glass by setting this tool or wire which made contact on the upper side, at a point offset from that on the other side, and the crack would join the two. I could cut bevel-edged discs in that way. The bevel was not perfectly smooth or regular, but was improved by simply grinding it a little.

THE CHAIRMAN:—The hour is growing late, and if there are any further remarks on this paper we would like to hear them. If not, I would like to call special attention to the evening session. There will be a paper read by Mr. Nikola Tesla, in Prof. Dwight's room at Columbia College, this evening at 8.30 p. m. The subject of this paper will be "Experiments with Alternating Currents of High Frequency." Many of those experiments have been produced before some of the gentlemen who are here at the present time, and they are of an exceedingly interesting character, and it is hoped that every one who is here now will be present at that lecture to-night. In the absence of the President, Prof. Bell, Mr. Lockwood will occupy the Chair.

The meeting then adjourned until 8.30 p. m.

EVENING SESSION,

AT COLUMBIA COLLEGE, NEW YORK CITY,

June 20th, 1891.

THE CHAIRMAN (Vice-President Thomas D. Lockwood):—I have the honor now of introducing to you Mr. Nikola Tesla, who will deliver a lecture on "Experiments with Alternating Currents of High Frequency." Members of the Institute will recollect that it was Mr. Tesla who first brought before the Institute and the public at large the possibility and probability of electric motors operated by alternating currents. I am quite sure that the lecture he has to give us this evening and the experiments which will illustrate it, will be worthy of our most earnest consideration and our most profound attention. I wish to call attention on this occasion to the fact that the circumstance that Mr. Tesla is not an American-born is only one more ingredient in the history of electricity which shows that electricity is cosmopolitan. The different mile stones of progress in electricity as applied to the arts, show us nothing more clearly than they show that. From the time when electricity was given its name by Gilbert, to the present, every civilized nation and many individuals of many nations have contributed to the grand whole.

A lecture delivered before the American Institute of Electrical Engineers, at Columbia College, N. Y., May 20th, 1891. Vice-President Lockwood in the Chair.

EXPERIMENTS WITH ALTERNATE CURRENTS OF VERY HIGH FREQUENCY AND THEIR APPLICATION TO METHODS OF ARTIFICIAL ILLUMINATION.

BY NIKOLA TESLA.

There is no subject more captivating, more worthy of study than nature. To understand this great mechanism, to discover the forces which are active, and the laws which govern them, is the highest aim of the intellect of man.

Nature has stored up in the universe infinite energy. The eternal recipient and transmitter of this infinite energy is the ether. The recognition of the existence of ether, and of the functions it performs, is one of the most important results of modern scientific research. The mere abandoning of the idea of action at a distance, the assumption of a medium pervading all space and connecting all gross matter, has freed the minds of thinkers of an ever present doubt and by opening a new horizon —new and unforeseen possibilities—has given fresh interest to phenomena with which we are familiar of old. It has been a great step towards the understanding of the forces of nature and their multifold manifestations to our senses. It has been for the enlightened student of physics what the understanding of the mechanism of the firearm or of the steam engine was for the barbarian. Phenomena upon which we used to look as wonders baffling explanation, we now see in a different light. The spark of an induction coil, the glow of an incandescent lamp, the manifestations of the mechanical forces of currents and magnets are no longer beyond our grasp; instead of the incomprehensible, as before, their observation suggests now in our

minds a simple mechanism, and although as to its precise nature all is still conjecture, yet we know that the truth cannot be much longer hidden, and instinctively we feel that the understanding is dawning upon us. We still admire these beautiful phenomena, these strange forces, but we are helpless no longer; we can in a certain measure explain them, account for them, and we are hopeful of finally succeeding in unraveling the mystery which surrounds them.

In how far we can understand the world around us is the ultimate thought of every student of nature. The coarseness of our senses prevents us from recognizing the ulterior construction of matter, and astronomy this grandest and most positive of natural sciences, can only teach us something that happens, as it were, in our immediate neighborhood; of the remoter portions of the boundless universe, with its numberless stars and suns, we know nothing. But far beyond the limit of perception of our senses the spirit still can guide us, and so we may hope, that even these unknown worlds—infinitely small and great—may in a measure become known to us. Still, even if this knowledge should reach us, the searching mind will find a barrier, perhaps forever unsurpassable, to *true* recognition of that which *seems* to be, the mere *appearance* of which is the only and slender basis of all our philosophy.

Of all the forms of nature's immeasurable, all-pervading energy, which ever and ever changing and moving, like a soul animates the inert universe, those of electricity and magnetism are perhaps the most fascinating. The effects of gravitation of heat and light we observe daily, and soon we get accustomed to them, and soon they lose for us the character of the marvelous and wonderful; but electricity and magnetism, with their singular relationship, with their seemingly dual character unique among the forces in nature, with their phenomena of attractions, repulsions and rotations, strange manifestations of mysterious agents, stimulate and excite the mind to thought and research. What is electricity, and What is magnetism? These questions have been asked again and again. The most able intellects have ceaselessly wrestled with the problem; still the question has not as yet been fully answered. But while we cannot even to-day state what these singular forces are, yet we have made good headway towards the solution of the problem. We are now confident that electric and magnetic phenomena are attributable to ether, and

we are perhaps justified in saying, that the effects of static electricity are effects of ether under strain and those of dynamic electricity and electro-magnetism effects of ether in motion. But this still leaves the question, as to what electricity and magnetism are, unanswered

First, we naturally inquire, What is electricity, and is there such a thing as electricity? In interpreting electric phenomena, we may speak of electricity or of an electric condition, state or effect. If we speak of electric effects, we must distinguish two such effects opposite in character and neutralizing each other, as observation shows that two such opposite effects exist. This is unavoidable, for in a medium of the properties of ether, we cannot possibly exert a strain, or produce a displacement or motion of any kind, without causing in the surrounding medium an equivalent and opposite effect. But if we speak of electricity, meaning a *thing*, we must, I think, abandon the idea of two electricities, as the existence of two such things is highly improbable. For how can we imagine that there should be two things, equivalent in amount, alike in their properties, but of opposite character, both clinging to matter, both attracting and completely neutralizing each other? Such an assumption, though suggested by many phenomena, though most convenient for explaining them, has little to commend it. If there *is* such a thing as electricity, there can be only *one* such thing, and, excess and want of that one thing, possibly, but more probably its condition determines the positive and negative character. The old theory of Franklin, though falling short in some respects, is, from a certain point of view, after all, the most plausible one. Still, in spite of this, the theory of the two electricities is generally accepted, as it apparently explains electric phenomena in a more satisfactory manner. But a theory which explains better the facts is not necessarily true. Ingenious minds will invent theories to suit observation, and almost every independent thinker has his own views on the subject.

It is not with the object of advancing an opinion, but with the desire of acquainting you better with some of the results, which I will describe, to show you the reasoning I have followed, the departures I have made—that I venture to express, in a few words, the views and convictions which have led me to these results.

I adhere to the idea that there is a thing which we have been in the habit of calling electricity. The question is, What is that

thing? or, What, of all things, the existence of which we know, have we the best reason to call electricity? We know that it acts like an incompressible fluid; that there must be a constant quantity of it in nature; that it can be neither produced nor destroyed; and, what is more important, the electro-magnetic theory of light and all facts observed teach us that electric and other phenomena are identical. The idea at once suggests itself, therefore, that electricity might be called ether. In fact, this view has in a certain sense been advanced by Dr. Lodge. His interesting work has been read by everyone and many have been convinced by his arguments. His great ability and the interesting nature of the subject, keep the reader spellbound; but when the impressions fade, one realizes that he has to deal only with ingenious explanations. I must confess, that I cannot believe in two electricities, much less in a doubly constituted ether. The puzzling behavior of the ether as a solid to waves of light and heat, and as a fluid to the motion of bodies through it, is certainly explained in the most natural and satisfactory manner, by assuming it to be in motion, as Sir William Thomson has suggested; but regardless of this, there is nothing which would enable us to conclude with certainty that, while a fluid is not capable of transmitting transverse vibrations of a few hundred or thousand per second, it might not be capable of transmitting such vibrations when they range into hundreds of million millions per second. Nor can any one prove that there are transverse ether waves emitted from an alternate current machine, giving a small number of alternations per second; to such slow disturbances, the ether, if at rest, may behave as a true fluid.

Returning to the subject, and bearing in mind that the existence of two electricities is, to say the least, highly improbable, we must remember, that we have no evidence of electricity, nor can we hope to get it unless gross matter is present. Electricity, therefore, cannot be called ether in the broad sense of the term; but nothing would seem to stand in the way of calling electricity ether associated with matter, or bound ether; or, in other words, that the so-called static charge of the molecule is ether associated in some way with the molecule. Looking at it in that light, we would be justified in saying, that electricity is concerned in all molecular actions.

Now, precisely what the ether surrounding the molecules is, wherein it differs from ether in general, can only be conjectured.

It cannot differ in density, ether being incompressible; it must, therefore, be under some strain or in motion, and the latter is the most probable. To understand its functions, it would be necessary to have an exact idea of the physical construction of matter, of which, of course, we can only form a mental picture.

But of all the views of nature, the one which assumes one matter and one force, and a perfect uniformity throughout, is the most scientific and most likely to be true. An infinitesimal world, with the molecules and their atoms spinning and moving in orbits, in much the same manner as celestial bodies, carrying with them and probably spinning with them ether, or in other words, carrying with them static charges, seems to my mind the most probable view, and one which, in a plausible manner, accounts for most of the phenomena observed. The spinning of the molecules and their ether sets up ether tensions or electrostatic strains; the equalization of ether tensions sets up ether motions or electric currents, and the orbital movements produce the effects of electro and permanent magnetism.

About fifteen years ago, Prof. Rowland demonstrated a most interesting and important fact, namely, that a static charge carried around produces the effects of an electric current. Leaving out of consideration the precise nature of the mechanism, which produces the attraction and repulsion of currents, and conceiving the electrostatically charged molecules in motion, this experimental fact gives us a fair idea of magnetism. We can conceive lines or tubes of force which physically exist; being formed of rows of directed moving molecules; we can see that these lines must be closed; that they must tend to shorten and expand, etc. It likewise explains in a reasonable way, the most puzzling phenomenon of all, permanent magnetism, and, in general, has all the beauties of the Ampere theory without possessing the vital defect of the same, namely, the assumption of molecular currents. Without enlarging further upon the subject, I would say, that I look upon all electrostatic current and magnetic phenomena as being due to electrostatic molecular forces.

The preceding remarks I have deemed necessary to a full understanding of the subject as it presents itself to my mind.

Of all these phenomena the most important to study are the current phenomena, on account of the already extensive and ever-growing use of current for industrial purposes. It is now a century since the first practical source of current has been produced,

and, ever since, the phenomena which accompany the flow of currents have been diligently studied, and through the untiring efforts of scientific men the simple laws which govern them have been discovered. But these laws were found to hold good only when the currents are of a steady character. When the currents are rapidly varying in strength quite different phenomena, often unexpected, present themselves, and quite different laws hold good, which even now have not been determined as fully as is desirable, though through the work principally of English scientists, enough knowledge has been gained on the subject to enable us to treat simple cases, which now present themselves in daily practice.

The phenomena which are peculiar to the changing character of the currents are greatly exalted when the rate of change is increased, hence the study of these currents is considerably facilitated by the employment of properly constructed apparatus. It was with this and other objects in view that I constructed alternate current machines capable of giving more than two million reversals of current per minute, and to this circumstance it is principally due that I am able to bring to your attention some of the results thus far reached, which I hope will prove to be a step in advance on account of their direct bearing upon one of the most important problems, namely, the production of a practical and efficient source of light.

The study of such rapidly alternating currents is very interesting. Nearly every experiment discloses something new. Many results may, of course, be predicted, but many more are unforeseen. The experimenter makes many interesting observations. For instance, we take a piece of iron and hold it against a magnet. Starting from low alternations and running up higher and higher, we feel the impulses succeed each other faster and faster, get weaker and weaker, and finally disappear. We then observe a continuous pull; the pull, of course, is not continuous; it only appears so to us—our sense of touch is imperfect.

We may next establish an arc between the electrodes and observe as the alternations rise, that the note which accompanies alternating arcs gets shriller and shriller, gradually weakens, and finally ceases. The air vibrations of course continue, but they are too weak to be perceived—our sense of hearing fails us.

We observe the small physiological effects, the rapid heating of the iron cores and conductors, curious inductive effects, interest-

ing condenser phenomena, and still more interesting light phenomena with a high tension induction coil. All these experiments and observations would be of the greatest interest to the student, but their description would lead me too far from the principal subject. Partly for this reason and partly on account of the vastly greater importance, I will confine myself to the description of the light effects produced by these currents.

In the experiments to this end, a high tension induction coil or equivalent apparatus for converting currents of comparatively low into currents of high tension is used.

If you will be sufficiently interested in the results I shall describe, as to enter into an experimental study of this subject; if you will be convinced of the truth of the arguments I shall advance, your aim will be to produce high frequencies and high potentials—in other words, powerful electrostatic effects. You will then encounter many difficulties which, if completely overcome, would allow us to produce truly wonderful results.

First will be met the difficulty of obtaining the required frequencies by means of mechanical apparatus, and if they be obtained otherwise, obstacles of a different nature will present themselves. Next, it will be found difficult to provide the requisite insulation without considerably increasing the size of the apparatus, for the potentials required are high, and owing to the rapidity of the alternations the insulation presents peculiar difficulties. So, for instance, when a gas is present, the discharge may work through by the molecular bombardment of the gas and consequent heating, through as much as an inch of the best solid insulating material, such as glass, hard rubber, porcelain, sealing wax, etc.; in fact, through any known insulating substance. The chief requisite in the insulation of the apparatus is, therefore, the exclusion of any gaseous matter.

In general, my experience tends to show that bodies which possess the highest specific inductive capacity such as glass, afford a rather inferior insulation to others which, while they are good insulators, have a much smaller specific inductive capacity, such as oils, for instance, the dielectric losses being no doubt greater in the former. The difficulty of insulating, of course, only exists when the potentials are excessively high, for with potentials such as a few thousand volts there is no particular difficulty encountered in conveying currents from a machine giving say 20,000 alternations per second, to quite a distance. This num-

ber of alternations, however, is by far too small for many purposes, though quite sufficient for some practical applications This difficulty of insulating is fortunately not a vital drawback; it affects mostly the size of the apparatus, for when excessively high potentials would be used, the light-giving devices would be located not far from the apparatus and often they would be quite close to it. As the air-bombardment of the insulated wire is dependent on condenser action, the loss may be reduced to a trifle by using excessively thin wires heavily insulated.

Another difficulty which will be encountered in the capacity and self-induction necessarily possessed by the coil. If the coil be large—that is, if it contain a great length of wire, it will be generally unsuited for excessively high frequencies; if it be small, it may be well adapted for such frequencies, but the potential might then not be as high as desired. A good insulator, and preferably one possessing a small specific inductive capacity, would afford a two-fold advantage. First, it would enable us to construct a very small coil capable of withstanding enormous differences of potential; and secondly, such a small coil, by reason of its smaller capacity and self-induction, would be capable of a quicker and more vigorous vibration. The problem, then, of constructing a coil or induction apparatus of any kind, possessing the requisite qualities, I regard as one of no small importance, and it has occupied me for a considerable time.

The investigator who desires to repeat the experiments which I will describe, with an alternate current machine capable of supplying currents of the desired frequency, and an induction coil, will do well to take the primary coil out and mount the secondary in such a manner as to be able to look through the tube upon which the secondary is wound. He will then be able to observe the streams which pass from the primary to the insulating tube, and from their intensity he will know how far he can strain the coil. Without this precaution, he is sure to injure the insulation. This arrangement permits, however, an easy exchange of the primaries, which is desirable in these experiments.

The selection of the type of machine best suited for the purpose, must be left to the judgment of the experimenter There are here illustrated three distinct types of machines which, besides others, I have used in my experiments.

Fig. 1 represents the machine used in my experiments before this Institute. The field magnet consists of a ring of wrought-

iron with 384 pole projections. The armature comprises a steel disc to which is fastened a thin, carefully-welded rim of wrought-iron. Upon the rim are wound several layers of fine, well-annealed iron wire which, when wound is passed through shellac. The armature wires are wound around brass pins wrapped with silk thread. The diameter of the armature wire in this type of machine should not be more than one-sixth of the thickness of the pole projections, else the local action will be considerable.

Fig. 2 represents a larger machine of a different type. The field magnet of this machine consists of two like parts which

FIG. 1.—High Frequency Alternator with Drum Armature.

either inclose an exciting coil or else are independently wound. Each part has 480 pole projections, the projections of one facing those of the other. The armature consists of a wheel of hard bronze, carrying the conductors which revolve between the projections of the field magnet. To wind the armature conductors, I have found it most convenient to proceed in the following manner: I construct a ring of hard bronze of the required size. This ring and the rim of the wheel are provided with the proper number of pins and both fastened upon a plate. The armature conductors being wound, the pins are cut off and the ends of the

conductors fastened by two rings which screw to the bronze ring and the rim of the wheel respectively. The whole may then be taken off and forms a solid structure. The conductors in such a type of machine should consist of sheet copper, the thickness of which, of course, depends on the thickness of the pole projections; or else twisted thin wires should be employed.

Fig. 3 is a smaller machine, in many respects similar to the former, only here the armature conductors and the exciting coil are kept stationary, while only a block of wrought-iron is revolved.

FIG. 2.—High Frequency Alternator with Revolving Disc Armature.

It would be uselessly lengthening this description were I to dwell more on the details of construction of these machines. Besides, they have been described somewhat more elaborately in the *Electrical Engineer* of March 18, 1891. I deem it well, however, to call the attention of the investigator to two things, the importance of which, though self-evident, he is nevertheless apt to underestimate, namely, to the local action in the conductors, which must be carefully avoided, and to the clearance, which must be small. I may add that since it is desirable to use very high peripheral speeds, the armature should be of very large

diameter in order to avoid impracticable belt speeds. Of the several types of these machines which have been constructed by me, I have found that the type illustrated in Fig. 1 caused me the least trouble in construction as well as in maintenance and, on the whole, it has been a good experimental machine.

In operating an induction coil with very rapidly alternating currents, among the first luminous phenomena noticed are naturally those presented by the high tension discharge. As the number of alternations per second is increased, or as—the number being high—the current through the primary is varied, the discharge gradually changes in appearance. It would be difficult to describe the minor changes which occur and the conditions

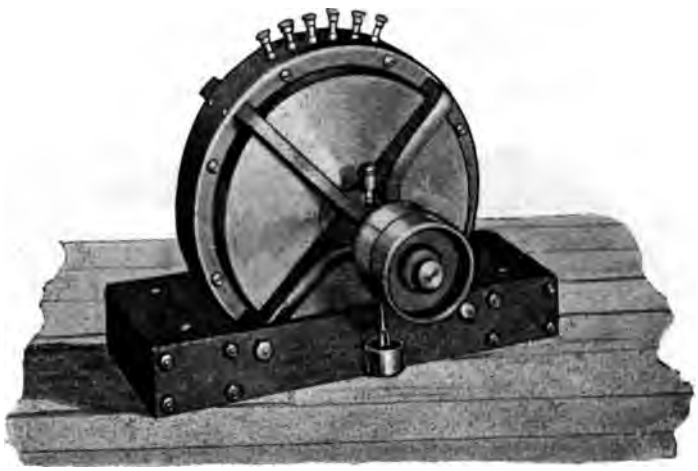

FIG. 3.—High Frequency Alternator with Stationary Disc Armature and Stationary Exciting Coil.

which bring them about, but one may note five distinct forms of the discharge.

First, one may observe a weak, sensitive discharge in the form of a thin feeble-colored thread. (Fig. 4.) It always occurs when, the number of alternations per second being high, the current through the primary is very small. In spite of the excessively small current, the rate of change is great and the difference of potential at the terminals of the secondary is therefore considerable, so that the arc is established at great distances; but the quantity of "electricity" set in motion is insignificant, barely sufficient to maintain a thin, threadlike arc. It is exces-

sively sensitive and may be made so to such a degree that the mere act of breathing near the coil will affect it, and unless it is perfectly well protected from currents of air, it wriggles around constantly. Nevertheless, it is in this form excessively persistent, and when the terminals are approached to, say one-third of the striking distance, it can be blown out only with difficulty. This exceptional persistency, when short, is largely due to the arc being excessively thin; presenting, therefore, a very small surface to the blast. Its great sensitiveness, when very long, is probably due to the motion of the particles of dust suspended in the air.

When the current through the primary is increased the discharge gets broader and stronger, and the effect of the capacity of the coil becomes visible until, finally, under proper conditions, a white flaming arc, Fig. 5, often as thick as one's finger and striking across the whole coil, is produced. It develops remarkable heat, and may be further characterized by the absence of

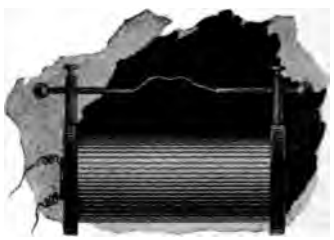

FIG. 4.—Sensitive Thread Discharge.

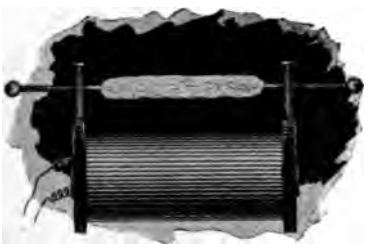

FIG. 5.—Flaming Discharge.

the high note which accompanies the less powerful discharges. To take a shock from the coil under these conditions would not be advisable, although under different conditions, the potential being much higher, a shock from the coil may be taken with impunity. To produce this kind of discharge the number of alternations per second must not be too great for the coil used; and, generally speaking, certain relations between capacity, self-induction and frequency must be observed.

The importance of these elements in an alternate current circuit is now well known, and under ordinary conditions the general rules are applicable. But in an induction coil exceptional conditions prevail. First, the self-induction is of little importance before the arc is established, when it asserts itself, but perhaps never as prominently as in ordinary alternate current-circuits, because capacity is distributed all along the coil, and by reason

of the fact that the coil usually discharges through very great resistances; hence the currents are exceptionally small. Secondly, the capacity goes on increasing continually as the potential rises, in consequence of absorption which takes place to a considerable extent. Owing to this there exists no critical relationship between these quantities, and ordinary rules would not seem to be applicable. As the potential is increased either in consequence of the increased frequency or of the increased current through the primary, the amount of the energy stored becomes greater and greater and the capacity gains more and more in importance. Up to a certain point the capacity is beneficial, but after that it begins to be an enormous drawback. It follows from this that each coil gives the best result with a given frequency and primary current. A very large coil when operated with currents of very high frequency may not give as much as $\frac{1}{8}$ inch spark. By adding capacity to the terminals the condition may be improved, but what the coil really wants is a lower frequency.

When the flaming discharge occurs, the conditions are evidently such that the greatest current is made to flow through the circuit. These conditions may be attained by varying the frequency within wide limits, but the highest frequency at which the flaming arc can still be produced determines, for a given primary current, the maximum striking distance of the coil. In the flaming discharge, the *eclat* effect of the capacity is not perceptible. The rate at which the energy is being stored then just equals the rate at which it can be disposed of through the circuit. This kind of discharge is the severest test for a coil; the break, when it occurs, is of the nature of that in an overcharged Leyden jar. To give a rough approximation, I would state that with an ordinary coil, of say 10,000 ohms resistance, the most powerful arc would be produced with about 12,000 alternations per second.

When the frequency is increased beyond that rate, the potential of course rises, but the striking distance may nevertheless diminish, paradoxical as it may seem. As the potential rises, the coil attains more and more the properties of a static machine, until, finally, one may observe the beautiful phenomenon of the streaming discharge, Fig. 6, which may be produced across the whole length of the coil. At that stage, streams begin to issue freely from all points and projections. These streams will also be seen to pass in abundance in the space between the primary and the

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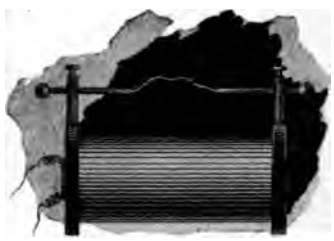
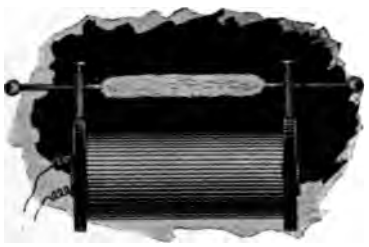


FIG. 4.—Sensitive Thread Discharge.



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insulating tube. When the potential is excessively high, they will always appear, even if the frequency be low, and even if the primary be surrounded by as much as an inch of wax, hard rubber, glass or any other insulating substance. This limits greatly the output of the coil, but I will later show how I have been able to overcome to a considerable extent this disadvantage in the ordinary coil.

Besides the potential, the intensity of the streams depends on the frequency; but if the coil be very large they show themselves no matter how low the frequencies used. For instance, in a very large coil of resistance of 67,000 ohms, constructed by me some time ago, they appear with as low as 100 alternations per second and less, the insulation of the secondary being $\frac{3}{4}$ inch of ebonite. When very intense, they produce a noise similar to that pro-

FIG. 6.—Streaming Discharge.

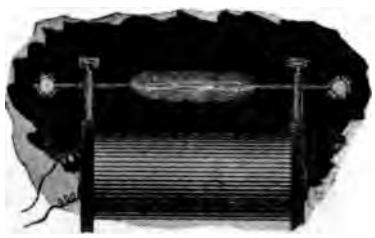

FIG. 7.—Brush and Spray Discharge.

duced by the charging of a Holtz machine, but much more powerful, and they emit a strong smell of ozone. The lower the frequency, the more apt they are to suddenly injure the coil. With excessively high frequencies, they may pass freely without producing any other effect than to heat the insulation slowly and uniformly.

The existence of these streams shows the importance of constructing an expensive coil, so as to permit of one's seeing through the tube surrounding the primary, and the latter should be easily exchangeable, or else the space between the primary and secondary should be completely filled up with insulating material, so as to exclude all air. The non-observance of this simple rule in the construction of the commercial coils, is responsible for the destruction of many an expensive coil.

At the stage when the streaming discharge occurs, or with somewhat higher frequencies, one may by approaching the terminals considerably and regulating properly the effect of capacity, produce a veritable spray of small silver-white sparks or a

bunch of excessively thin silvery threads [Fig. 7] amidst a powerful brush—each spark or thread possibly corresponding to one alternation. This, when produced under proper conditions, is probably the most beautiful discharge, and when an air blast is directed against it, it presents a singular appearance. The spray of sparks, when received through the body, causes some inconvenience, whereas when the discharge simply streams, nothing at all is likely to be felt if large conducting objects are held in the hands to protect them from receiving small burns.

If the frequency is still more increased, then the coil refuses to give any spark unless at comparatively small distances, and the fifth typical form of discharge may be observed [Fig. 8]. The tendency to stream out and dissipate is then so great that when the brush is produced at one terminal, no sparking occurs even if, as I have repeatedly tried, the hand, or any conducting object, is

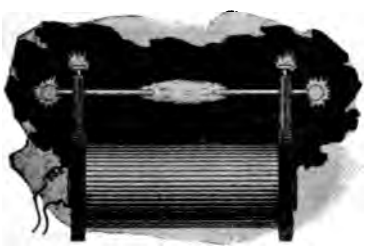

FIG. 8.—Fifth Typical Form of Discharge.

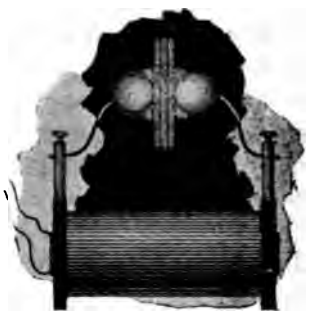

FIG. 9.—Luminous Discharge with Interposed Insulators.

held within the stream; and what is more singular, the luminous stream is not at all easily deflected by the approach of a conducting body.

At this stage, the streams seemingly pass with the greatest freedom through considerable thicknesses of insulators, and it is particularly interesting to study their behavior. For this purpose it is convenient to connect to the terminals of the coil two metallic spheres, which may be placed at any desired distance [Fig. 9.] Spheres are preferable to plates, as the discharge can be better observed. By inserting dielectric bodies between the spheres, beautiful discharge phenomena may be observed. If the spheres be quite close and a spark be playing between them, by interposing a thin plate of ebonite between the spheres the spark instantly ceases and the discharge spreads into an intensely lumi-

nous circle several inches in diameter, provided the spheres are sufficiently large. The passage of the streams heats and, after a while, softens the rubber so much that two plates may be made to stick together in this manner. If the spheres are so far apart that no spark occurs even if they are far beyond the striking distance, by inserting a thick plate of glass the discharge is instantly induced to pass from the spheres to the glass in the form of luminous streams. It appears almost as though these streams pass *through* the dielectric. In reality this is not the case, as the streams are due to the molecules of the air which are violently agitated in the space between the oppositely charged surfaces of the spheres. When no dielectric other than air is present, the bombardment goes on but is too weak to be visible; by inserting a dielectric, the inductive effect is much increased and, besides, the projected air molecules find an obstacle and the bombardment becomes so intense that the streams become luminous. If by any mechanical means we could effect such a violent agitation of the molecules, we could produce the same phenomenon. A jet of air escaping through a small hole under enormous pressure and striking against an insulating substance, such as glass, may be luminous in the dark, and it might be possible to produce phosphorescence of the glass or other insulators in this manner.

The greater the specific inductive capacity of the interposed dielectric, the more powerful the effect produced. Owing to this, the streams show themselves with excessively high potentials even if the glass be as much as one and one-half to two inches thick. But besides the heating due to bombardment, some heating goes on undoubtedly in the dielectric, being apparently greater in glass than in ebonite. I attribute this to the greater specific inductive capacity of the glass, in consequence of which, with the same potential difference, a greater amount of energy is taken up in it than in rubber. It is like connecting to a battery a copper and a brass wire of the same dimensions. The copper wire, though a more perfect conductor, would heat more by reason of its taking more current. Thus what is otherwise considered a virtue of the glass, is here a defect. Glass usually gives way much quicker than ebonite; when it is heated to a certain degree, the discharge suddenly breaks through at one point, assuming then the ordinary form of an arc.

The heating effect produced by molecular bombardment of the dielectric would, of course, diminish as the pressure of the air is

increased, and at enormous pressure it would be negligible, unless the frequency would increase correspondingly.

It will be often observed in these experiments that when the spheres are beyond the striking distance, the approach of a glass plate, for instance, may induce the spark to jump between the spheres. This occurs when the capacity of the spheres is somewhat below the critical value which gives the greatest difference of potential at the terminals of the coil. By approaching a dielectric the specific inductive capacity of the space between the spheres is increased, producing the same effect as if the capacity of the spheres were increased. The potential at the terminals may then rise so high that the space is cracked. The experiment is best performed with dense glass or mica.

Another interesting observation is that a plate of insulating material, when the discharge is passing through it, is strongly attracted by either of the spheres, that is, by the nearer one, this being obviously due to the smaller mechanical effect of the bombardment on that side, and perhaps also to the greater electrification.

From the behavior of the dielectrics in these experiments, we may conclude that the best insulator for these rapidly alternating currents would be the one possessing the smallest specific inductive capacity and at the same time one capable of withstanding the greatest differences of potential; and thus two diametrically opposite ways of securing the required insulation are indicated, namely, to use either a perfect vacuum or a gas under great pressure, but the former would be preferable. Unfortunately, neither of these two ways is easily carried out in practice.

It is especially interesting to note the behavior of an excessively high vacuum in these experiments. If a test tube, provided with external electrodes, and exhausted to the highest possible degree, be connected to the terminals of the cell, Fig. 10, the electrodes of the tube are instantly brought to a high temperature and the glass at each end of the tube is rendered intensely phosphorescent, but the middle appears comparatively dark and for a while remains cool.

When the frequency is so high that the discharge shown in Fig. 8 is observed, considerable dissipation no doubt occurs in the coil. Nevertheless, the coil may be worked for a long time, as the heating is gradual.

In spite of the fact that the difference of potential may be enor-

mous, little is felt when the discharge is passed through the body, provided the hands are armed. This is to some extent due to the higher frequency, but principally to the fact that less energy is available externally, when the difference of potential reaches an enormous value, owing to the circumstance that with the rise of potential the energy absorbed in the coil increases as the square of the potential. Up to a certain point, the energy available externally, increases with the rise of potential, then it begins to fall off rapidly. Thus, with the ordinary high-tension induction coil, the curious paradox exists that while with a given current through the primary the shock might be fatal, with many times that current it might be perfectly harmless, even if the frequency be the same. With high frequencies and excesssively high potentials, when the terminals are not connected to bodies of some size, practically all the energy supplied to the primary is taken

FIG. 10.—Discharge Through Highest Vacuum.

FIG. 11.—Pinwheel driven by a Powerful Brush.

up by the coil. There is no breaking through, no local injury, but all the material, insulating and conducting, is uniformly heated.

To avoid misunderstanding in regard to the physiological effect of alternating currents of very high frequency, I think it necessary to state that while it is an undeniable fact that they are incomparably less dangerous than currents of low frequencies, yet it should not be thought that they are altogether harmless. What has just been said refers only to currents from an ordinary high-tension induction coil, which currents are necessarily very small; if received directly from a machine or from a secondary of low resistance, they produce more or less powerful effects and may cause serious injury, especially when used in conjunction with condensers.

The streaming discharge of a high-tension induction coil differs in many respects from that of a powerful static machine. In color it has neither the violet of the positive nor the brightness of the negative static discharge, but lies somewhere between, being, of course, alternatively positive and negative. But since the streaming is more powerful when the point or terminal is electrified positively than when electrified negatively, it follows that the point of the brush is more like the positive and the root more like the negative static discharge. In the dark, when the brush is very powerful, the root may appear almost white. The wind produced by the escaping streams, though it may be very strong—often, indeed, to such a degree that it may be felt quite a distance from the coil—is, nevertheless, considering the quantity of the discharge, smaller than that produced by the positive

FIG. 12.—Luminous Streams Escaping From a Cotton-Covered Wire.

FIG. 13.—Aspect Presented by a Very Thin Wire Attached to a Terminal of the Coil.

brush of a static machine, and it affects the flame much less powerfully. From the nature of the phenomenon, we can conclude that the higher the frequency, the smaller must, of course, be the wind produced by the streams, and with sufficiently high frequencies no wind at all would be produced at the ordinary atmospheric pressures. With frequencies obtainable by means of a machine, the mechanical effect is sufficiently great to revolve with considerable speed large pin-wheels, which in the dark present a beautiful appearance, owing to the abundance of the streams [Fig. 11].

In general, most of the experiments usually performed with a static machine can be performed with an induction coil when operated with very rapidly alternating currents. The effects pro-

duced, however, are much more striking, being of incomparably greater power. When a small length of ordinary cotton-covered wire [Fig. 12] is attached to one terminal of the coil, the streams issuing from all points of the wire may be so intense as to produce a considerable light effect. When the potentials, and frequencies are very high, a wire insulated with gutta-percha or rubber and attached to one of the terminals, appears to be covered with a luminous film. A very thin bare wire when attached to a terminal emits powerful streams and vibrates continually to and fro, or spins in a circle, producing a singular effect [Fig. 13]. Some of these experiments have been described by me in the *Electrical World* of February 21, 1891.

Another peculiarity of the rapidly alternating discharge of the induction coil, is its radically different behavior with respect to points and rounded surfaces.

If a thick wire, provided with a ball at one end and with a point at the other, be attached to the positive terminal of a static machine, practically all the charge will be lost through the point, on account of the enormously greater tension, dependent on the radius of curvature. But if such a wire is attached to one of the terminals of the induction coil, it will be observed that with very high frequencies streams issue from the ball almost as copiously as from the point [Fig. 14].

It is hardly conceivable that we could produce such a condition to an equal degree in a static machine, for the simple reason that the tension increases as the square of the density, which in turn is proportional to the radius of curvature. Hence, with a steady potential, an enormous charge would be required to make streams issue from a polished ball while it is connected with a point. But with an induction coil, the discharge of which alternates with great rapidity, it is different. Here we have to deal with two distinct tendencies. First, there is a tendency to escape which exists in a condition of rest and which depends on the radius of curvature; second, there is the tendency to dissipate into the surrounding air by condenser action, which depends on the surface. When one of these tendencies is at a maximum, the other is at a minimum. At the point, the luminous stream is principally due to the air molecules coming bodily in contact with the point; they are attracted and repelled, charged and discharged, and their atomic charges being thus disturbed, vibrate and emit light waves. At the ball, on the contrary, there is no

doubt that the effect is to a great extent produced inductively, the air molecules not necessarily coming in contact with the ball, though they undoubtedly do so. To convince ourselves of this, we need only to exalt the condenser action, for instance, by enveloping the ball at some distance by a better conductor than the surrounding medium, the conductor being, of course, insulated; or else by surrounding it with a better dielectric and approaching an insulated conductor. In both cases the streams will break forth more copiously. Also the larger the ball with a given frequency, or the higher the frequency, the more will the ball have the advantage over the point. But since a certain intensity of action is required to render the streams visible, it is obvious that in the experiment described the ball should not be taken too large.

In consequence of this two-fold tendency, it is possible to pro-

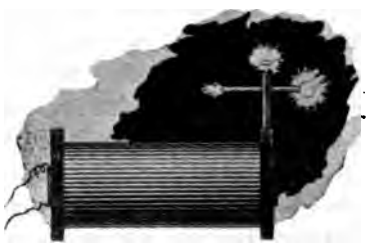

FIG. 14.—Effect of Ball and Point.

FIG. 15.—Aspect of Coil under Powerful Brush Discharge.

duce by means of points, effects identical to those produced by capacity. Thus, for instance, by attaching to one terminal of the coil a small length of coiled wire, presenting many points and offering great facility to escape, the potential of the coil may be raised to the same value as by attaching to the terminal a polished ball of a surface many times greater than that of the wire.

An interesting experiment, showing the effect of the points, may be performed in the following manner: Attach to one of the terminals of the coil a cotton covered wire, about two feet in length, and adjust the conditions so that streams issue from the wire. In this experiment the primary coil should be preferably placed so that it extends only about half way into the secondary coil. Now touch the free terminal of the secondary with a conducting object held in the hand, or else connect it to an insulated

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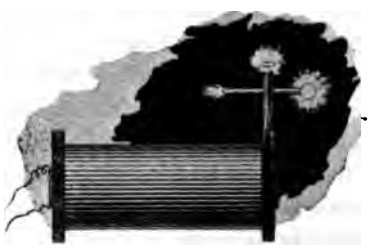

FIG. 14.—Effect of Ball and Point.

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There is still another and far more striking peculiarity of the brush discharge produced by very rapidly alternating currents. To observe this it is best to replace the usual terminals of the coil by two metal columns insulated with a good thickness of ebonite. It is also well to close all fissures and cracks with wax so that the brushes cannot form anywhere except at the tops of the columns. If the conditions are carefully adjusted—which, of course, must be left to the skill of the experimenter—so that the potential rises to an enormous value, one may produce two powerful brushes several inches long, nearly white at their roots, which in the dark bear a striking resemblance to two flames of a gas escaping under pressure [Fig. 15]. But they do not only *resemble*, they *are* veritable flames, for they are hot. Certainly they are not as hot as a gas flame, *but they would be so if the frequency and the potential should be sufficiently high.* Produced with, say twenty thousand alternations per second, the heat is easily perceptible, even if the potential is not excessively high. The heat developed is, of course, due to the impact of the air molecules against the terminals and against each other. As, at the ordinary pressures, the mean free path is excessively small, it is possible that in spite of the enormous initial speed imparted to each molecule upon coming in contact with the terminal, its progress—by collision with other molecules—is retarded to such an extent, that it does not get away far from the terminal, but may strike the same many times in succession. The higher the frequency the less the molecule is able to get away, and this the more so, as for a given effect the potential required is smaller; and a frequency is conceivable —perhaps even obtainable—at which practically the same molecules would strike the terminal. Under such conditions the ex-

change of the molecules would be very slow, and the heat produced at and very near the terminal would be excessive. But if the frequency would go on increasing constantly, the heat produced would begin to diminish for obvious reasons. In the positive brush of a static machine the exchange of the molecules is very rapid, the stream is constantly of one direction, and there are fewer collisions; hence the heating effect must be very small. Anything that impairs the facility of exchange tends to increase the local heat produced. Thus, if a bulb be held over the terminal of the coil so as to enclose the brush, the air contained in the bulb is very quickly brought to a high temperature. If a glass tube be held over the brush so as to allow the draught to carry the brush upward, scorching hot air escapes at the top of the tube. Anything held within the brush is, of course, rapidly heated, and the possibility of using such heating effects for some purpose or other suggests itself.

When contemplating this singular phenomenon of the hot brush, we cannot help being convinced that a similar process must take place in the ordinary flame, and it seems strange that after all these centuries past, of familiarity with the flame, now, in the era of electric lighting and heating, we are finally led to recognize that since time immemorial we have after all, always had "electric light and heat" at our disposal. It is also of no little interest to contemplate that we have a possible way of producing —by other than chemical means—a veritable flame, which would give light and heat without any material being consumed, without any chemical process taking place, and to accomplish this we only need to perfect methods of producing enormous frequencies and potentials. I have no doubt that if the potential could be made to alternate with sufficient rapidity and power, the brush formed at the end of a wire would lose its electrical characteristics and would become flamelike. The flame must be due to electrostatic molecular action.

This phenomenon now explains in a manner which can hardly be doubted, the frequent accidents occurring in storms. It is well known that objects are often set on fire without the lightning striking them. We shall presently see how this can happen. On a nail in a roof, for instance, or on a projection of any kind more or less conducting or rendered so by dampness, a powerful brush may appear. If the lightning strikes somewhere in the neighborhood, the enormous potential may be made to alternate or fluctu-

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ate perhaps many million times a second. The air molecules are violently attracted and repelled, and by their impact produce such a powerful heating effect that a fire is started. It is conceivable that a ship at sea may in this manner catch fire at many points at once. When we consider that even with the comparatively low frequencies obtained from a dynamo machine, and with potentials of no more than one or two hundred thousand volts, the heating effects are considerable, we may imagine how much more powerful they must be with frequencies and potentials many times greater, and the above explanation seems, to say the least, very probable. Similar explanations may have been suggested, but I am not aware that up to the present the heating effects of a brush produced by a rapidly alternating potential, have been experimentally demonstrated, at least not to such a remarkable degree.

FIG. 16.—Incandescent Wire or Filament Spinning in an Unexhausted Globe.

By preventing completely the exchange of the air molecules, the local heating effect may be so exalted as to bring a body to incandescence. Thus, for instance, if a small button or, preferably, a very thin wire or filament be enclosed in an unexhausted globe and connected with the terminal of the coil, it may be rendered incandescent. The phenomenon is made much more interesting by the rapid spinning around in a circle of the top of the filament, thus presenting the appearance of a luminous funnel, [Fig. 16], which widens when the potential is increased. When the potential is small, the end of the filament may perform irregular motions, suddenly changing from one to the other, or it may describe an ellipse But when the potential is very high it always spins in a circle, and so does generally a thin straight wire attached freely to the terminal of the coil. These motions are,

of course, due to the impact of the molecules and the irregularity in the distribution of the potential owing to the roughness and dis-symmetry of the wire or filament. With a perfectly symmetrical and polished wire such motions would probably not occur. That the motion is not likely to be due to other causes is evident from the fact that it is not of a definite direction and that in a very highly exhausted globe it ceases altogether. The possibility of bringing a body to incandescence in an unexhausted globe, or even when not at all enclosed, would seem to afford a possible way of obtaining lighter effects, which in perfecting methods of producing rapidly alternating potentials might be rendered available for useful purposes.

In employing a commercial coil, the production of very powerful brush effects is attended with considerable difficulties, for when these high frequencies and enormous potentials are used,

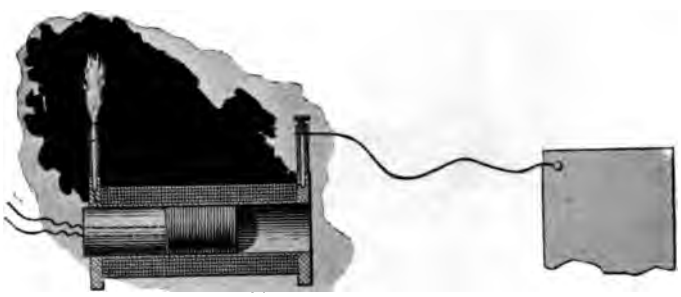

FIG. 17A.—Coil Arranged for Powerful Brush Effects.

the best insulation is apt to give way. Usually the coil is insulated well enough to stand the strain from convolution to convolution, since two double silk covered paraffined wires will withstand a pressure of several thousand volts; the difficulty lies principally in preventing the breaking through from the secondary to the primary, which is greatly facilitated by the streams issuing from the latter. In the coil, of course, the strain is greatest from section to section, but usually in a larger coil there are so many sections that the danger of a sudden giving way is not very great. No difficulty will generally be encountered in that direction, and besides the liability of injuring the coil internally is very much reduced by the fact that the effect most likely to be produced is simply a gradual heating, which, when far enough advanced, could not fail to be observed. The principal necessity is then, to prevent the streams between the primary and the tube,

not only on account of the heating and possible injury, but also because the streams may diminish very considerably the potential difference available at the terminals. A few hints as to how this may be accomplished will probably be found useful in most of these experiments with the ordinary induction coil.

One of the ways is to wind a short primary, [Fig. 17 A], so that the difference of potential is not at that length great enough to cause the breaking forth of the streams through the insulating tube. The length of the primary should be determined by experiment. Both the ends of the coil should be brought out on one end through a plug of insulating material fitting in the tube as illustrated. In such a disposition, one terminal of the secondary is attached to a body, the surface of which is determined with the greatest care so as to produce the greatest rise in the potential.

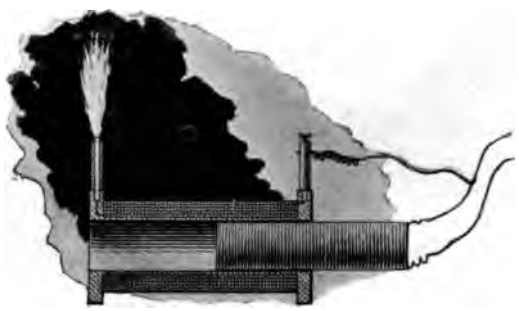

FIG. 17B.—Coil Arranged for Powerful Brush Effects.—St. Elmo's Hot Fire.

At the other terminal a powerful brush appears which may be experimented upon.

The above plan necessitates the employment of a primary of comparatively small size, and it is apt to heat when powerful effects are desirable for a certain length of time. In such a case it is better to employ a larger coil, [Fig. 17 B], and introduce it from one side of the tube, until the streams begin to appear. In this case the nearest terminal of the secondary may be connected to the primary or to the ground, which is practically the same thing, if the primary is connected directly to the machine. In the case of ground connections it is well to determine experimentally the frequency which is best suited under the conditions of the test. Another way of obviating the streams, more or less, is to make the primary in sections and supply it from separate well insulated sources.

In many of these experiments, when powerful effects are wanted for a short time it is advantageous to use iron cores with the primaries In such case a very large primary coil may be wound and placed side by side with the secondary, and the nearest terminal of the latter being connected to the primary, a laminated iron core is introduced through the primary into the secondary as far as the streams will permit. Under these conditions an excessively powerful brush, several inches long, which may be appropriately called "St Elmo's hot fire," may be caused to appear at the other terminal of the secondary, producing striking effects. It is a most powerful ozonizer, so powerful indeed, that only few minutes are sufficient to fill the whole room with the smell of ozone, and it undoubtedly possesses the quality of exciting chemical affinities.

For the production of ozone, alternating currents of very high frequency are eminently suited, not only on account of the advantages they offer in the way of conversion, but also because of the fact, that the ozonizing action of a discharge is dependent on the frequency as well as on the potential, this being undoubtedly confirmed by observation.

In these experiments if an iron core is used, it should be carefully watched, as it is apt to get excessively hot in an incredibly short time. To give an idea of the rapidity of the heating, I will state, that by passing a powerful current through a coil with many turns, the inserting within the same of a thin iron wire for no more than one second's time is sufficient to heat the wire to something like 100° C.

But this rapid heating does not need to discourage us in the use of iron cores in connection with rapidly alternating currents. I have for a long time been convinced, that in the industrial distribution by means of transformers some such plan as the following might be practicable. We may use a comparatively small iron core, subdivided, or perhaps not even subdivided. We may surround this core with a considerable thickness of material which is fire proof and conducts the heat poorly, and on top of that we may place the primary and secondary windings. By using either higher frequencies or greater magnetizing forces, we may by hystersis and eddy currents heat the iron core so far as to bring it nearly to its maximum permeability, which, as Hopkinson has shown, may be as much as sixteen times greater than that at ordinary temperatures. If the iron core were perfectly enclosed it

would not be deteriorated by the heat, and if the enclosure of fire proof material would be sufficiently thick only a limited amount of energy could be radiated in spite of the high temperature. Transformers have been constructed by me on that plan, but for lack of time, no thorough tests have as yet been made.

Another way of adapting the iron core to rapid alternations. or, generally speaking, reducing the frictional losses, is to produce by continuous magnetization a flow of something like seven thousand or eight thousand lines per square centimetre through the core, and then work with weak magnetizing forces and preferably high frequency around the point of greatest permeability. A higher efficiency of conversion and greater output are obtainable in this manner. I have also employed this principle in con-

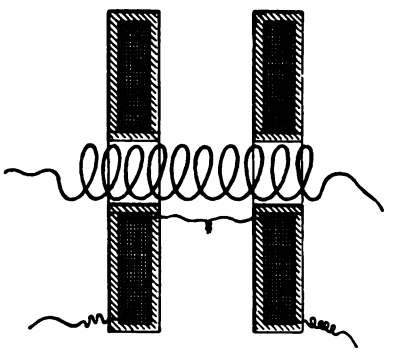

FIG. 18.—Coil for Producing Very High Difference of Potential.

nection with machines in which there is no reversal of polarity. In these types of machines, as long as there are only few pole projections, there is no great gain, as the maxima and minima of magnetization are far from the point of maximum permeability, but when the number of the pole projections is very great, the required rate of change may be obtained, without the magnetization varying so far as to depart greatly from the point of maximum permeability, and the gain is considerable.

The above-described arrangements refer only to the use of commercial coils as ordinarily constructed. If it is desired to construct a coil for the express purpose of performing with it such experiments as I have described, or generally rendering it capable of withstanding the greatest possible difference of potential, then a construction as indicated in Fig. 18, will be found of ad-

vantage. The coil in this case is formed of two independent parts which are wound oppositely, the connection between both being made near the primary. The potential in the middle being zero, there is not much tendency to jump to the primary and not much insulation is required. In some cases the middle point may, however, be connected to the primary or to the ground. In such a coil the places of greatest difference of potential are far apart and the coil is capable of withstanding an enormous strain. The two parts may be movable so as to allow a slight adjustment of the capacity effect.

As to the manner of insulating the coil, it will be found convenient to proceed in the following way: First, the wire should be boiled in paraffin until all the air is out; then the coil is wound by running the wire through melted paraffin, merely for the purpose of fixing the wire. The coil is then taken off from the spool, immersed in a cylindrical vessel filled with pure melted wax and boiled for a long time, until the bubbles cease to appear. The whole is then left to cool down thoroughly, and then the mass is taken out of the vessel and turned up in a lathe. A coil made in this manner and with care, is capable of withstanding enormous potential differences.

It may be found convenient to immerse the coil in paraffin oil or some other kind of oil; it is a most effective way of insulating, principally on account of the perfect exclusion of air, but it may be found that after all a vessel filled with oil is not a very convenient thing to handle in a laboratory.

If an ordinary coil can be dismounted, the primary may be taken out of the tube and the latter plugged up on one end, filled with oil, and the primary reinserted. This affords an excellent insulation and prevents the formation of the streams.

Of all the experiments which may be performed with rapidly alternating currents, the most interesting are those which concern the production of a practical luminant. It cannot be denied that the present methods, though they were brilliant advances, are very wasteful. Some better methods must be invented—some more perfect apparatus devised. Modern research has opened new possibilities for the production of an efficient source of light and the attention of all has been turned in the direction indicated by able pioneers. Many have been carried away by the enthusiasm and passion to discover, but in their zeal to reach results many have been misled. Starting with the idea of producing

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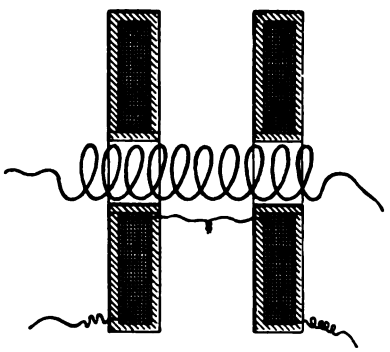

FIG. 18.—Coil for Producing Very High Difference of Potential.

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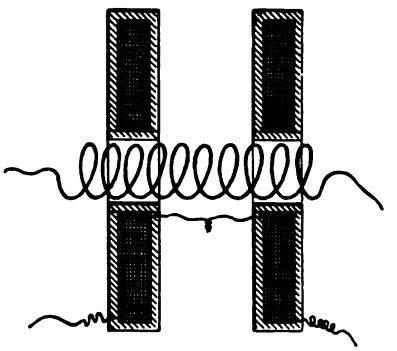

FIG. 18.—Coil for Producing Very High Difference of Potential.

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electro-magnetic waves, they turned their attention perhaps too much to the study of electro-magnetic effects and neglected the study of electrostatic phenomena. Naturally, nearly ever investigator availed himself of an apparatus similar to that used in earlier experiments. But in those forms of apparatus, while the electro-magnetic inductive effects are enormous, the electrostatic effects are excessively small.

In the Hertz experiments, for instance, a high-tension induction coil is short-circuited by an arc, the resistance of which is very small; the smaller, the more capacity is attached to the terminals; and the difference of potential at these is enormously diminished. On the other hand, when the discharge is not passing between the terminals the static effects may be considerable, but only qualitively so, not quantitatively, since their rise and fall is very sudden and since their frequency is small. In neither case, therefore, are powerful electrostatic effects perceivable. Similar conditions exist when, as in some interesting experiments of Dr. Lodge, Leyden jars are discharged disruptively. It has been thought—and I believe asserted—that in such cases most of the energy is radiated into space. In the light of the experiments, which I have described above, it will now not be thought so. I feel safe in asserting, that in such cases most of the energy is partly taken up and converted into heat in the arc of the discharge and in the conducting and insulating material of the jar, some energy being, of course, given off by electrification of the air; but the amount of the directly radiated energy is very small.

When a high-tension induction coil, operated by currents alternating only 20,000 times a second, has its terminals closed through even a very small jar, practically all the energy passes through the dielectric of the jar, which is heated, and the electrostatic effects manifest themselves outwardly only to a very weak degree. Now the external circuit of a Leyden jar, that is, the arc and the connections of the coatings may be looked upon as a circuit, generating alternating currents of excessively high frequency and fairly high potential, which is closed through the coatings and the dielectric between them, and from the above it is evident that the external electrostatic effects must be very small, even if a recoil circuit be used. These conditions make it appear, that with the apparatus usually at hand the observation of powerful electrostatic effects was impossible, and what

experience has been gained in that direction is only due to the great ability of the investigators.

But powerful electrostatic effects are *sine qua non* of light productions on the lines indicated by theory. Electro-magnetic effects are primarily unavailable, for the reason that to produce the required effects we would have to pass current impulses through a conductor which, long before the required frequency of the impulses could be reached, would cease to transmit them. On the other hand, electro-magnetic waves many times longer than those of light and producible by sudden discharges of a condenser could not be utilized, it would seem, except we avail ourselves of their effect upon conductors as in the present methods, which are wasteful. We could not affect by means of such waves, the static molecular or atomic charges of a gas, cause them to vibrate and to emit light. Long transverse waves cannot, apparently, produce such effects, since excessively small electro-magnetic disturbances may pass readily through miles of air. Such dark waves, unless they are of the length of true light waves, cannot, it would seem, excite luminous radiation in a Geissler tube, and the luminous effect, which are producible by induction in a tube devoid of electrodes, I am inclined to consider as being of an electrostatic nature.

To produce such luminous effects, straight electrostatic thrusts are required; these, whatever be their frequency, may disturb the molecular charges and produce light. Since current impulses of the required frequency cannot pass through a conductor of measurable dimensions, we must work with a gas and then the production of powerful electrostatic effects becomes an imperative necessity.

It has occurred to me, however, that electrostatic effects are in many ways available for the production of light. For instance, we may place a body of some refractory material in a closed and, preferably, more or less exhausted globe, connect it to a source of high, rapidly alternating potential, causing the molecules of the gas to strike it many times a second at enormous speeds, and in this manner, with trillions of invisible hammers, pound it until it gets incandescent; or we may place a body in a very highly exhausted globe, in a non-striking vacuum, and by employing very high frequencies and potentials, transfer sufficient energy from it to other bodies in the vicinity or in general to the surroundings, to maintain it at any degree of incandescence; or we may, by

means of such rapidly alternating high potentials, disturb the ether carried by the molecules of a gas or their static charges, causing them to vibrate and to emit light.

But electrostatic effects being dependent upon the potential and frequency to produce the most powerful action, it is desirable to increase both as far as practicable. It may be possible to obtain quite fair results by keeping either of these factors small, provided the other is sufficiently great; but we are limited in both directions. My experience demonstrates that we cannot go below a certain frequency, for, first, the potential then becomes so great that it is dangerous; and, secondly, the light production is less efficient.

I have found that by using the ordinary low frequencies, the physiological effect of the current required to maintain, at a certain degree of brightness, a tube four feet long, provided at the ends with outside and inside condenser coatings, is so powerful that I think it might produce serious injury to those not accustomed to such shocks; whereas, with twenty thousand alternations per second the tube may be maintained at the same degree of brightness without any effect being felt. This is due principally to the fact that a much smaller potential is required to produce the same light effect, and also to the higher efficiency in the light production. It is evident that the efficiency in such cases is the greater the higher the frequency, for the quicker the process of charging and discharging the molecules, the less energy will be lost in the form of dark radiation. But, unfortunately, we can not go beyond a certain frequency, on account of the difficulty of producing and conveying the effects.

I have stated above, that a body enclosed in an unexhausted bulb may be intensely heated by simply connecting it with a source of rapidly alternating potential. The heating in such a case is in all probability due mostly to the bombardment of the molecules of the gas contained in the bulb. When the bulb is exhausted, the heating of the body is much more rapid and there is no difficulty whatever in bringing a wire or filament to any degree of incandescence by simply connecting it to one terminal of a coil of the proper dimensions. Thus, if the well known apparatus of Prof. Crookes, consisting of a bent platinum wire with vanes mounted over it [Fig. 19], be connected to one terminal of the coil—either one or both ends of the platinum wire being connected—the wire is rendered almost instantly incandescent and

the mica vanes are rotated as though a current from a battery were used. A thin carbon filament or, preferably, a button of some refractory material [Fig. 20], even if it be a comparatively poor conductor, enclosed in an exhausted globe, may be rendered highly incandescent; and in this manner a simple lamp capable of giving any desired candle power is provided.

The success of lamps of this kind would depend largely on the selection of the light-giving bodies contained within the bulb. Since, under the conditions described, refractory bodies—which are very poor conductors and capable of withstanding for a long time excessively high degrees of temperature—may be used, such illuminating devices may be rendered successful.

It might be thought at first that if the bulb containing the filament or button of refractory material, be perfectly well exhausted

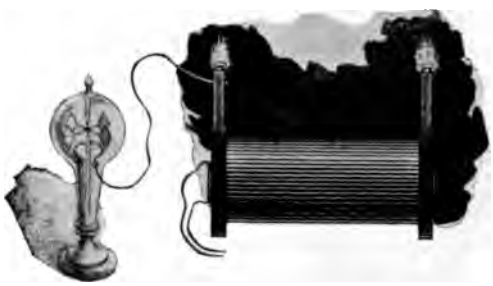

FIG. 19.—The Crookes Experiment on Open Circuit.

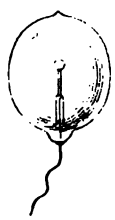

FIG. 20.—Lamp with Single Block of Refractory Material.

—that is, as far as it can be done by the use of the best apparatus—the heating would be much less intense, and that in perfect vacuum it could not occur at all. This is not confirmed by my experience. Quite the contrary; the better the vacuum, the easier the bodies are brought to incandescence. This result is interesting for many reasons.

At the outset of this work, the idea presented itself to me, whether two bodies of refractory material enclosed in a bulb, exhausted to such a degree that the discharge of a large induction coil operated in the usual manner cannot pass through, could be rendered incandescent by mere condenser action. Obviously, to reach this result enormous potential differences and very high frequencies are required, as is evident from a simple calculation.

But such a lamp would possess a vast advantage over an ordinary incandescent lamp in regard to efficiency. It is well known that the efficiency of a lamp is to some extent a function of the degree of incandescence and that could we but work a filament at many times higher degrees of incandescence the efficiency would be much greater. In an ordinary lamp this is impracticable, on account of the destruction of the filament, and it has been determined by experience how far it is advisable to push the incandescence. It is impossible to tell how much higher efficiency could be obtained if the filament could withstand indefinitely, as the investigation to this end obviously cannot be carried beyond a certain stage; but there are reasons for believing that it would be very considerably higher. An improvement might be made in the ordinary lamp by employing a short and thick carbon; but then the leading-in wires would have to be thick and, besides, there are many other considerations which render such a modification entirely impracticable. But in a lamp as above described, the leading-in wires may be very small, the incandescent refractory material may be in the shape of blocks offering a very small radiating surface, so that less energy would be required to keep them at the desired incandescence; and in addition to this, the refractory material need not be carbon, but may be manufactured from mixtures of oxides, for instance, with carbon or other material, or may be selected from bodies which are practically non-conductors and capable of withstanding enormous degrees of temperature.

All this would point to the possibility of obtaining a much higher efficiency with such a lamp than is obtainable in ordinary lamps. In my experience it has been demonstrated that the blocks are brought to high degrees of incandescence with much lower potentials than those determined by calculation, and the blocks may be set at greater distances from each other. We may freely assume, and it is probable, that the molecular bombardment is an important element in the heating, even if the globe be exhausted with the utmost care, as I have done; for although the number of the molecules is, comparatively speaking, insignificant, yet on account of the mean free path being very great, there are fewer collisions and the molecules may reach much higher speeds, so that the heating effect due to this cause may be considerable, as in the Crookes experiments with radiant matter.

But it is likewise possible that we have to deal here with an

increased facility of losing the charge in very high vacuum, when the potential is rapidly alternating, in which case most of the heating would be directly due to the surging of the charges in heated bodies Or else the observed fact may be largely attributable to the effect of the points which I have mentioned above, in consequence of which the blocks or filaments contained in the vacuum are equivalent to condensers of many times greater surface than that calculated from their geometrical dimensions. Scientific men still differ in opinion as to whether a charge should, or should not, be lost in a perfect vacuum, or in other words, whether ether is, or is not, a conductor. If the former were the case, then a thin filament enclosed in a perfectly

FIG. 21.—Lamp with Two Filaments in Highest Vacuum with Leading-in Wires.

exhausted globe and connected to a source of enormous, steady potential would be brought to incandescence.

Various forms of lamps on the above described principle, with the refractory bodies, in the form of filaments Fig. 21, or blocks, Fig. 22, have been constructed and operated by me, and investigations are being carried on in this line. There is no difficulty in reaching such high degrees of incandescence that ordinary carbon is to all appearance melted and volatilized. If the vacuum could be made absolutely perfect, such a lamp, although inoperative with apparatus ordinarily used, would, if operated with currents of the required character, afford an illuminant which would never be destroyed and which would be far more efficient than an ordinary incandescent lamp. This perfection can, of course, never

be reached and a very slow destruction and gradual diminution of the size always occurs, as in incandescent lamps; but there is no possibility of a sudden and premature disabling which occurs in the latter by the breaking of the filament, especially when the incandescent bodies are in the shape of blocks.

With these rapidly alternating potentials there is, however, no necessity of enclosing two blocks in a globe, but a single block, as in Fig. 20, or filament, Fig. 23, may be used. The potential in this case must of course be higher, but it is easily obtainable and besides it is not necessarily dangerous.

The facility with which the button or filament in such a

FIG. 22.—Lamp with Two Refractory Blocks in Highest Vacuum.

FIG. 23.—Lamp with Single Straight Filament and one Leading-in Wire.

lamp is brought to incandescence, other things being equal, depends on the size of the globe. If a perfect vacuum could be obtained, the size of the globe would not be of importance, for then the heating would be wholly due to the surging of the charges and all the energy would be given off to the surroundings by radiation. But this can never occur in practice. There is always some gas left in the globe and although the exhaustion may be carried to the highest degree, still the space inside of the bulb must be considered as conducting when such high potentials are used, and I assume, that in estimating the energy that may be given off from the filament to the surroundings, we may consider

the inside surface of the bulb as one coating of a condenser, the air and other objects surrounding the bulb forming the other coating. When the alternations are very low there is no doubt that a considerable portion of the energy is given off by the electrification of the surrounding air.

In order to study this subject better, I carried on some experiments with excessively high potentials and low frequencies. I then observed that when the hand is approached to the bulb,—the filament being connected with one terminal of the coil,—a powerful vibration is felt, being due to the attraction and repulsion of the molecules of the air which are electrified by induction through the glass. In some cases when the action is very intense I have been able to hear a sound, which must be due to the same cause.

FIG. 24.—Lamps wtih One Leading in Wire Rendered Incandescent.

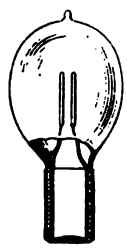

FIG. 25.—Lamp with two Blocks or Filaments and a Pair of Independent Inside and Outside Condenser Coatings.

When the alternations are low, one is apt to get an excessively powerful shock from the bulb. In general, when one attaches bulbs or objects of some size to the terminals of the coil, one should look out for the rise of potential, for it may happen that by merely connecting a bulb or plate to the terminal, the potential may rise to many times its original value. When lamps are attached to the terminals, as illustrated in Fig. 24, then the capacity of the bulbs should be such as to give the maximum rise of potential under the existing conditions. In this manner one may obtain the required potential with fewer turns of wire.

The life of such lamps as described above depends, of course, largely on the degree of exhaustion, but to some extent also on the shape of the block of refractory material. Theoretically it would seem, that a small sphere of carbon enclosed in a sphere of

glass would not suffer deterioration from molecular bombardment, for the matter in the globe, being radiant, the molecules would move in straight lines and would seldom strike the sphere obliquely. An interesting thought in connection with such a lamp is, that in it "electricity" and electrical energy apparently must move in the same lines.

The use of alternating currents of very high frequency makes it possible to transfer by electrostatic or electromagnetic induction through the glass of a lamp sufficient energy to keep a filament at incandescence and so do away with the leading-in wires.

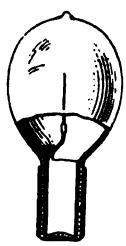

FIG. 26A. FIG. 26B.
Lamp with One Filament, One Inside and One Outside Condenser Coating.

Such lamps have been proposed, but for want of proper apparatus they have not been successfully operated. Many forms of lamps on this principle with continuous and broken filaments have been constructed by me and experimented upon. When using a secondary enclosed within the lamp a condenser is advantageously combined with the secomdary. When the transference is effected by electrostatic induction the potentials used are, of course, very high with frequencies obtainable from a machine. For instance, with a condenser surface of forty centimetres square, which is not

impracticably large, and with glass of good quality, 1 mm. thick, using currents alternating twenty thousand times a second, the potential required is approximately 9,000 volts. This may seem large, but since each lamp may be included in the secondary of a transformer of very small dimensions it would not be inconvenient-and, moreover, it would not produce fatal injury. The transform, ers would all be preferably in series. The regulation would offer no difficulties, as with currents of such frequencies it is very easy to maintain a constant current.

In the accompanying engravings some of the types of lamps of this kind are shown. Fig. 25 is such a lamp with a broken filament, and Fig 26 A and 26 B one with a single inside and outside coat-

FIG. 27.—Lamp with One Filament and Leading-in Wire and External Condenser Coating.

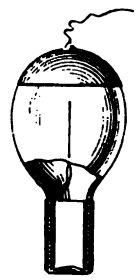

FIG. 28.—Lamp with One Filament, One Inside and One Outside Condenser Coating, and Auxiliary Coating.

ing and a single filament. I have also made lamps with two outside and inside coatings and a continuous loop connecting the latter. Such lamps have been operated by me with current impulses of the enormous frequencies obtainable by the disruptive discharge of condensers.

The disruptive discharge of a condenser is especially suited for operating such lamps—with no outward electrical connections—by means of electro-magnetic induction, the electro-magnetic inductive effects being excessively high; and I have been able to produce the desired incandescence with only a few short turns of wire. Incandescence may also be produced in this manner in a simple closed filament.

Leaving, now, out of consideration the practicability of such lamps, I would only say that they possess a beautiful and desirable feature, namely, that they can be rendered, at will, more or less brilliant simply by altering the relative position of the outside and inside condenser coatings or inducing and induced circuits.

When a lamp is lighted by connecting it to one terminal only of the source, this may be facilitated by providing the globe with an outside condenser coating, which serves at the same time as a reflector, and connecting this to an insulated body of some size. Lamps of this kind are illustrated in Fig. 27 and Fig. 28. Fig. 29 shows the plan of connections. The brilliancy of the lamp may in this case be regulated within wide limits by varying the size of the insulated metal plate to which the coating is connected.

It is likewise practicable to light with one leading wire, lamps

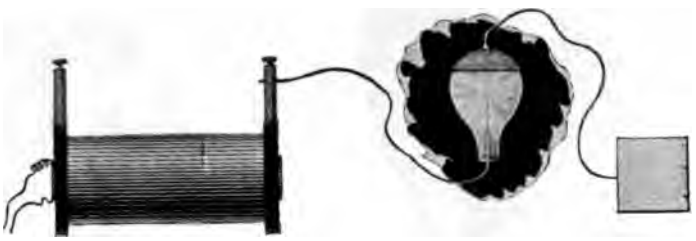

FIG. 29.—Increasing the Brilliancy of Lamp on One Wire.

such as illustrated in Fig. 21 and Fig. 22, by connecting one terminal of the lamp to one terminal of the source and the other to an insulated body of the required size. In all cases the insulated body serves to give off the energy into the surrounding space and is equivalent to a return wire. Obviously, in the two last-named cases, instead of connecting the wires to an insulated body, connections may be made to the ground.

The experiments which will prove most suggestive and of most interest to the investigator, are probably those performed with exhausted tubes. As might be anticipated, a source of such rapidly alternating potentials is capable of exciting the tubes at a considerable distance and the light effects produced are remarkable.

During my investigations in this line, I endeavored to excite tubes devoid of any electrodes, by electromagnetic induction, making the tube the secondary of the induction device and pass-

ing through the primary the discharges of a Leyden jar. These tubes were made of many shapes and I was able to obtain luminous effects which I then thought were due wholly to electromagnetic induction. But on carefully investigating the phenomena, I found that the effects produced were more of an electrostatic nature. It may be attributed to this circumstance that this mode of exciting tubes is very wasteful, namely, the primary circuit being closed, the potential, and consequently the electrostatic inductive effect, is much diminished.

When an inductive coil, operated as above described, is used, there is no doubt that the tubes are excited by electrostatic induction and that electromagnetic induction has little, if anything, to do with the phenomena.

This is evident from many experiments. For instance, if a tube be taken in one hand, the observer being near the coil, it is brilliantly lighted and remains so no matter in what position it is held relatively to the observer's body. Were the action electromagnetic, the tube could not be lighted when the observer's body is interposed between it and the coil, or at least its luminosity should be considerably diminished. When the tube is held exactly over the centre of the coil—the latter being wound in sections and the primary placed symmetrically to the secondary—it may remain completely dark, whereas it is rendered intensely luminous by moving it slightly to the right or left from the centre of the coil. It does not light because in the middle, both halves of the coil neutralize each other and the electric potential is zero. If the action were electromagnetic, the tube should light best in the plane through the centre of the coil, since the electromagnetic effect there should be a maximum. When an arc is established between the terminals, the tubes and lamps in the vicinity of the coil go out, but light up again when the arc is broken, on account of the rise of potential. Yet the electromagnetic effect should be practically the same in both cases.

By placing a tube at some distance from the coil and nearer to one terminal—preferably at a point on the axis of the coil—one may light it by touching the remote terminal with an insulated body of some size or with the hand, thereby raising the potential at that terminal nearer to the tube. If the tube is shifted nearer to the coil, so that it is lighted by the action of the nearer terminal, it may be made to go out by holding on an insulated support the end of a wire connected to the remote terminal, in the

vicinity of the nearer terminal, by this means counteracting the action of the latter upon the tube. These effects are evidently electrostatic. Likewise, when a tube is placed at a considerable distance from the coil, the observer may, standing upon an insulated support between coil and tube, light the latter by approaching the hand to it; or he may even render it luminous by simply stepping between it and the coil. This would be impossible with electromagnetic induction, for the body of the observer would act as a screen.

When the coil is energized by excessively weak currents, the experimenter may, by touching one terminal of the coil with the tube, extinguish the latter, and may again light it by bringing it out of contact with the terminal and allowing a small arc to form. This is clearly due to the respective lowering and raising of the potential at that terminal In the above experiment, when the tube is lighted through a small arc it may go out when the arc is broken, because the electrostatic inductive effect alone is too weak though the potential may be much higher; but when the arc is established, the electrification of the end of the tube is much greater, and it consequently lights.

If a tube is lighted by holding it near to the coil, and in the hand which is remote, by grasping the tube anywhere with the other hand, the part between the hands is rendered dark, and the singular effect of wiping out the light of the tube may be produced by passing the hand quickly along the tube and at the same time withdrawing it slightly from the coil, judging properly the distance, so that the tube remains dark afterwards.

If the primary coil is placed sidewise, as in Fig. 17 B, for instance, and an exhausted tube be introduced from the other side in the hollow space, the tube is lighted most intensely because of the increased condenser action, and in this position the striæ are most sharply defined. In all these experiments described and in many others, the action is clearly electrostatic.

The effects of screening also indicate the electrostatic nature of the phenomena and show something of the nature of electrification through the air. For instance, if a tube be placed in the direction of the axis of the coil and an insulated metal plate be interposed the tube will generally increase in brilliancy, or if it be too far from the coil to light, it may even be rendered luminous by interposing an insulated metal plate. The magnitude of

the effects depends to some extent on the size of the plate. But if the metal plate be connected by a wire to the ground its interposition will always make the tube go out even if it be very near the coil. In general, the interposition of a body between the coil and tube increases, or diminishes, the brilliancy of the tube or its facility to light up, according to whether it increases or diminishes the electrification. When experimenting with an insulated plate, the plate should not be taken too large, else it will generally produce a weakening effect by reason of its great facility for giving off energy to the surroundings.

If a tube be lighted at some distance from the coil and a plate of hard rubber or other insulating substance be interposed, the tube may be made to go out. The interposition of the dielectric in this case only slightly increases the inductive effect, but diminishes considerably the electrification through the air.

In all the cases, then, when we excite luminosity in exhausted tubes by means of such a coil the effect is due to the rapidly alternating electrostatic potential; and, furthermore, it must be attributed to the harmonic alternation produced directly by the machine and not to any superimposed vibration which might be thought to exist. Such superimposed vibrations are impossible when we work with an alternate current machine. If a spring be gradually tightened and released it does not perform independent vibrations; for this a sudden release is necessary. So with the alternate currents from a dynamo machine; the medium is harmonically strained and released, this giving rise to only one kind of waves; a sudden contact or break, or a sudden giving way of the dielectric, as in the disruptive discharge of a Leyden jar, are essential for the production of superimposed waves.

In all the last described experiments, tubes devoid of any electrodes may be used and there is no difficulty in producing by their means sufficient light to read by. The light effect is, however, considerably increased by the use of phosphorescent bodies such as yttria, uranium glass, etc. A difficulty will be found when the phosphorescent material is used, for with these powerful effects, it is carried gradually away, and it is preferable to use material in the form of a solid.

Instead of depending on induction at a distance, to light the tube, the same may be provided with an external—and, if desired, also with an internal—condenser coating and may then be suspended anywhere in the room from a conductor connected to one

terminal of the coil, and in this manner a soft illumination may be provided.

The ideal way of lighting a hall or room would, however, be to produce such a condition in it that an illuminating device

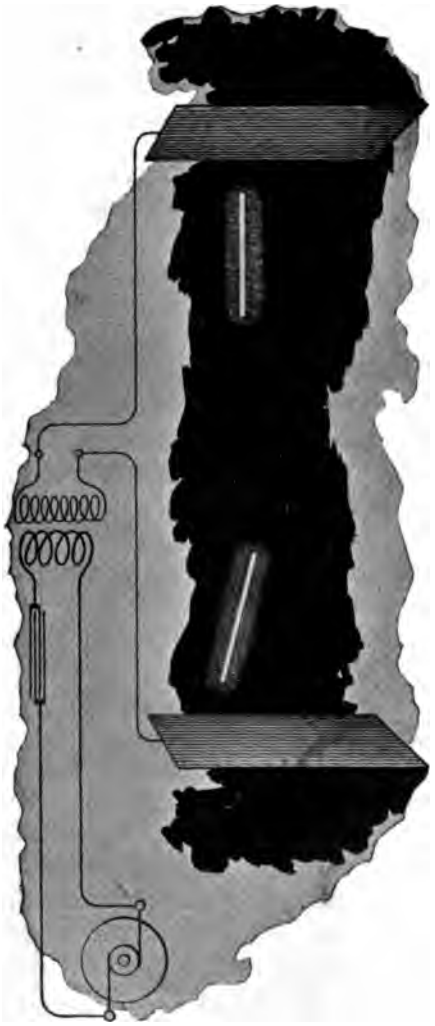

FIG. 30.—Ideal Method of Lighting a Room —Tubes Devoid of Any Electrodes Rendered Brilliant in an Alternating Electrostatic Field.

could be moved and put anywhere and that it is lighted, no matter where it is put and without being electrically connected to anything. I have been able to produce such a condition by creating in the room a powerful, rapidly alternating electrostatic

field. For this purpose I suspend a sheet of metal a distance from the ceiling on insulating cords and connect it to one terminal of the induction coil, the other terminal being preferably connected to the ground. Or else I suspend two sheets as illustrated in Fig. 30, each sheet being connected with one of the terminals of the coil, and their size being carefully determined. An exhausted tube may then be carried in the hand anywhere between the sheets or placed anywhere, even a certain distance beyond them; it remains always luminous.

In such an electrostatic field interesting phenomena may be observed, especially if the alternations are kept low and the potentials excessively high. In addition to the luminous phenomena mentioned, one may observe that any insulated conductor gives sparks when the hand or another object is approached to it, and the sparks may often be powerful. When a large conducting object is fastened on an insulating support and the hand approached to it, a vibration due to the rythmical motion of the air molecules is felt, and luminous streams may be perceived when the hand is held near a pointed projection. When a telephone receiver is made to touch with one or both of its terminals an insulated conductor of some size, the telephone emits a loud sound; it also emits a sound when a length of wire is attached to one or both terminals, and with very powerful fields a sound may be perceived even without any wire.

How far this principle is capable of practical application, the future will tell. It might be thought that electrostatic effects are unsuited for such action at a distance. Electromagnetic inductive effects, if available for the production of light, might be thought better suited. It is true the electrostatic effects diminish nearly with the cube of the distance from the coil, whereas the electromagnetic inductive effects diminish simply with the distance. But when we establish an electrostatic field of force the condition is very different, for then instead of the differential effect of both the terminals, we get their conjoint effect. Besides, I would call attention to the fact that in an alternating electrostatic field, a conductor, such as an exhausted tube, for instance, tends to take up most of the energy, whereas in an electromagnetic alternating field the conductor tends to take up the least energy, the waves being reflected with but little loss. This is one reason why it is difficult to excite an exhausted tube at a distance by electromagnetic induction. I have wound coils of very large

diameter and of many turns of wire, and connected a Geissler tube to the ends of the coil with the object of exciting the tube at a distance; but even with the powerful inductive effects producible by Leyden jar discharges, the tube could not be excited unless at a very small distance, although some judgment was used as to the dimensions of the coil. I have also found that even the most powerful Leyden jar discharges are capable of exciting only feeble luminous effects in a closed exhausted tube, and even these effects, upon thorough examination, I have been forced to consider of an electrostatic nature.

How, then, can we hope to produce the required effects at a distance by means of electromagnetic action, when even in the closest proximity to the source of disturbance, under the most advantageous conditions, we can excite but faint luminosity? It is true that when acting at a distance, we have the resonance to help us out. We can connect an exhausted tube or whatever the illuminating device may be, with an insulated system of the proper capacity, and so it may be possible to increase the effect qualitatively, and only qualitatively, for we would not get more energy through the device. So we may, by resonance effect, obtain the required electromotive force in an exhausted tube and excite faint luminous effects, but we cannot get enough energy to render the light practically available, and a simple calculation, based on experimental results, shows that even if all the energy which a tube would receive at a certain distance from the source, would be wholly converted into light, it would hardly satisfy the practical requirements. Hence the necessity of directing, by means of a conducting circuit, the energy to the place of transformation. But in so doing, we cannot very sensibly depart from present methods, and all we could do would be to improve the apparatus.

From these considerations, it would seem that if this ideal way of lighting is to be rendered practicable, it will be only by the use of electrostatic effects. In such a case, the most powerful electrostatic inductive effects are needed; the apparatus employed must, therefore, be capable of producing high electrostatic potentials changing in value with extreme rapidity. High frequencies are especially wanted, for practical considerations make it desirable to keep down the potential. By the employment of machines or, generally speaking, of any mechanical apparatus, but low frequencies can be reached; recourse must, therefore, be had to some

other means. The discharge of a condenser affords us a means of obtaining frequencies by far higher than are obtainable mechanically, and I have accordingly employed condensers in the experiments to the above end.

When the terminals of a high-tension induction coil [Fig. 31] are connected to a Leyden jar and the latter is discharging disruptively into a circuit, we may look upon the arc playing between the knobs as being a source of alternating or, generally speaking, undulating currents, and then we have to deal with the familiar system of a generator of such currents, a circuit connected to it and a condenser bridging the circuit. The condenser in such case is a veritable transformer, and since the frequency is excessive, most any ratio in the strength of the currents in both the branches may be obtained. In reality the analogy is not quite complete, for in the disruptive discharge we have most generally a fundamental instantaneous variation of comparatively low frequency

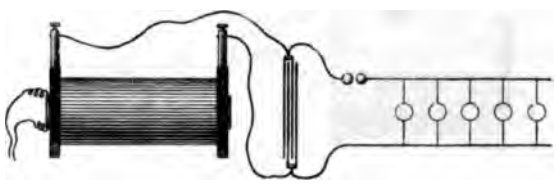

FIG. 31—Diagram of Connections for Converting from High to Low Tension by Means of the Disruptive Discharge.

and a superimposed harmonic vibration, and the laws governing the flow of currents are not the same for both.

In converting in this manner, the ratio of conversion should not be too great, for the loss in the arc between the knobs increases with the square of the current, and if the jar be discharged through very thick and short conductors, with the view of obtaining a very rapid oscillation, a very considerable portion of the energy stored is lost. On the other hand, too small ratios are not practicable for many obvious reasons.

As the converted currents flow in a practically closed circuit, the electrostatic effects are necessarily small, and I therefore convert them into currents or effects of the required character. I have effected such conversions in several ways. The preferred plan of connections is illustrated in Fig. 32. The manner of operating renders it easy to obtain, by means of a small and inexpensive apparatus, enormous differences of potential which have been usually

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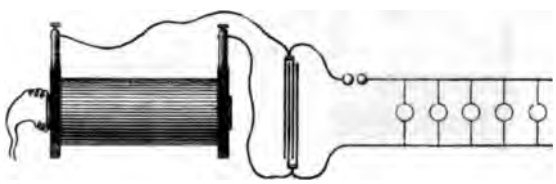

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obtained by means of large and expensive coils. For this it is only necessary to take an ordinary small coil, adjust to it a condenser and discharging circuit forming the primary of an auxiliary small coil, and convert upwards. As the inductive effect of the primary currents is excessively great, the second coil need have comparatively but very few turns. By properly adjusting the element, remarkable results may be secured.

In endeavoring to obtain the required electrostatic effects in this manner, I have, as might be expected, encountered many difficulties which I have been gradually overcoming, but I am not as yet prepared to dwell upon my experiences in this direction.

I believe that the disruptive discharge of a condenser will play an important part in the future, for it offers vast possibilities not only in the way of producing light in a more efficient manner and in line indicated by theory, but also in many other respects.

For years the efforts of inventors have been directed towards

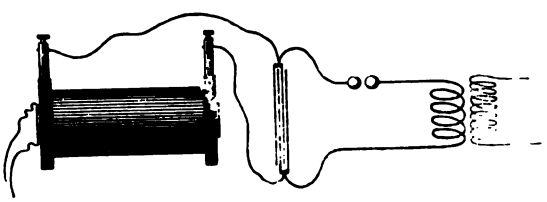


FIG. 29.—Manner of Operating an Induction Coil

obtaining electrical energy from heat by means of the thermopile. It might seem invidious to remark that but few know what is the real trouble with the thermopile. It is not the inefficiency or small output—though these are great drawbacks—but the fact that the thermopile has its phylloexra--that is, that by constant use it is deteriorated, which has thus far prevented its introduction on an industrial scale. Now that all modern research seems to point with certainty to the use of electricity of excessively high tension, the question must present itself to many whether it is not possible to obtain in a practicable manner this form of energy from heat. We have been used to look upon an electrostatic machine as a plaything, and somehow we couple with it the idea of the inefficient and impractical. But now we must think differently, for now we know that everywhere we have to deal with the same forces and that it is a mere question of inventing proper methods or apparatus for rendering them available.

In the present systems of electrical distribution the employment

of the iron with its wonderful magnetic properties, allows us to reduce considerably the size of the apparatus; but in spite of this it is still very cumbersome. The more we progress in the study of electric and magnetic phenomena, the more we become convinced that the present methods will be short lived. For the production of light at least, such heavy machinery would seem to be unnecessary. The energy required is very small, and if light can be obtained as efficiently, as theoretically it appears possible, the apparatus need have but a very small output. There being a strong probability that the illuminating methods of the future will involve the use of very high potentials, it seems very desirable to perfect a contrivance capable of converting the energy of heat into energy of the requisite form. Nothing to speak of has been done towards this end, for the thought that electricity of some 50,000 or 100,000 volts' pressure or more, even if obtained would be unavailable for practical purposes, has deterred inventors from working in this direction.

In Fig. 31, a plan of connections is shown for converting currents of high into currents of low tension, by means of the disruptive discharge of a condenser. This plan has been used by me frequently for operating a few incandescent lamps required in the laboratory. Some difficulties have been encountered in the arc of the discharge, which I have been able to overcome to a great extent; besides this and the adjustment necessary for the proper working, no other difficulties have been met with, and it was easy to operate ordinary lamps and even motors in this manner. The line being connected to the ground, all the wires could be handled with perfect impunity, no matter how high the potentials at the terminals of the condenser. In these experiments, a high-tension induction coil, operated from a battery or from an alternate current machine, was employed to charge the condenser; but the induction coil might be replaced by an apparatus of a different kind, capable of giving electricity of such high tension. In this manner direct or alternating currents may be converted, and in both cases the current-impulses may be of any desired frequency. When the currents charging the condenser are of the same direction, and it is desired that the converted currents should also be of one direction, the resistance of the discharging circuit should, of course, be so chosen that there are no oscillations.

In operating devices on the above plan, I have observed curious phenomena of impedance which are of interest. For instance, if

a thick copper bar be bent as indicated in Fig. 33, and shunted by ordinary incandescent lamps, then by passing the discharge between the knobs, the lamps may be brought to incandescence, although they are short-circuited. When a large induction coil is employed, it is easy to obtain nodes on the bar which are rendered evident by the different degree of brilliancy of the lamps, as shown roughly in Fig. 33. The nodes are never clearly pronounced, but there are simply maxima and minima of potential along the bar. This is probably due to the irregularity of the

FIG. 33.—Lamp Kept at Incandescence across a Thick Copper Bar—Showing Nodes.

arc between the knobs. In general, when the above-described plan of conversion from high to low tension is used, the behavior of the disruptive discharge may be closely studied. The nodes may also be investigated by means of an ordinary Cardew voltmeter, which should be well insulated. Geissler-tubes may also be lighted across the points of the bent bar. In this case, of course, it is better to employ smaller capacities. I have found it practicable to light up in this manner a lamp and even a Geissler tube shunted by a short, heavy block of metal, and this result seems at first very curious. In fact, the thicker the copper bar in Fig. 33,

the better it is for the success of the experiments, as they appear more striking. When lamps with long slender filaments are used, it will be often noted that the filaments are from time to time violently vibrated, the vibration being smallest on the nodal points. This vibration seems to be due to an electrostatic action between the filament and the glass of the bulb.

In some of the above experiments it is preferable to use special lamps having a straight filament, as shown in Fig. 34. When such a lamp is used a still more curious phenomenon than those described may be observed. The lamp may be placed across the copper bar and lighted, and by using somewhat larger capacities or, in other words, smaller frequencies, or smaller impulsive impedances, the filament may be brought to any desired degree of incandescence. But when the impedance is increased, a point is reached when comparatively little current

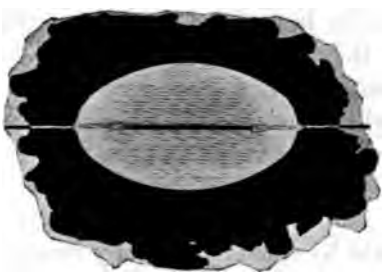


FIG. 34.—Phenomenon of Impedance in an Incandescent Lamp.

passes through the carbon and most of it through the rarefied gas, or perhaps it may be more correct to state that the current divides nearly evenly through both, in spite of the enormous difference in the resistance, and this would be true unless the gas and the filament behave differently. It is then noted that the whole bulb is brilliantly illuminated and the ends of the leading-in wires become incandescent and often throw off sparks in consequence of the violent bombardment; but the carbon filament remains dark. This is illustrated in Fig. 34. Instead of the filament, a single wire extending through the whole bulb may be used, and in this case the phenomenon would seem to be still more interesting.

From the above experiment it will be evident that when ordinary lamps are operated by the converted currents, those should be preferably taken in which the platinum wires are far apart and

the frequencies used should not be too great, else the discharge will occur at the ends of the filament or in the base of the lamp between the leading-in wires, and the lamp might then be damaged.

In presenting to you these results of my investigation on the subject under consideration, I have paid only a passing notice to facts upon which I could have dwelt at length, and among many observations I have selected only those which I thought most likely to interest you. The field is wide and completely unexplored, and at every step a new truth is gleaned, a novel fact observed.

How far the results here borne out are capable of practical applications, will be decided in the future. As regards the production of light, some results already reached are encouraging and make me confident in asserting that the practical solution of the problem lies in the direction I have endeavored to indicate. Still, whatever may be the immediate outcome of these experiments, I am hopeful that they will only prove a step to further development towards the ideal and final perfection. The possibilities which are opened by modern research are so vast that even the most reserved must feel sanguine of the future. Eminent scientists consider the problem of utilizing one kind of radiation without the others, a rational one. In an apparatus designed for the production of light by conversion of any form of energy into that of light, such a result can never be reached, for no matter what the process of producing the required vibrations, be it electrical, chemical or any other, it will not be possible to obtain the higher light vibrations without going through the lower heat vibrations. It is the problem of imparting to a body a certain velocity without passing through all lower velocities. But there is a possibility of obtaining energy not only in the form of light, but motive power and energy of any other form in some more direct way from the medium. The time will be when this will be accomplished, and the time has come when one may utter such words before an enlightened audience, without being considered a visionary. We are whirling through the endless space with an inconceivable speed; all around us everything is spinning, everything is moving, everywhere is energy. There *must* be some way of availing ourselves of this energy more directly; then, with the light obtained from the medium, with the power derived from it, with every

form of energy obtained without effort from the store forever inexhaustible, humanity will advance with giant strides. The mere contemplation of these magnificent possibilities expands our minds, strengthens our hopes and fills our hearts with supreme delight.

GENERAL MEETING,

THURSDAY, MAY 21ST, 1891.

MORNING SESSION.

The meeting was called to order at 11 A. M. Vice-President Lockwood in the Chair.

THE SECRETARY :—Mr. Chairman, there is an item of business referred to in the programme which I will explain, and then offer a resolution. The programme says that an official invitation has been extended to the Institute, to take part in the coming International Electrical Congress, to be held in September. Several of our members expect to attend the Frankfort Exhition this summer, among whom are Mr. Hering of Philadelphla and Dr. Nichols. It is uncertain at this time how many will be there at the date of this Electrical Congress. The proposition is that the President be authorized to appoint delegates to attend this Congress and then afterward to name such persons as it will be found convenient to have represent us on account of their being on the ground. I therefore move that three or five delegates be appointed by the Chair at its convenience to represent the American Institute of Electrical Engineers at the Electrical Congress to be held at Frankfort, Germany.

PROF. ANTHONY :—I second the motion.

THE CHAIRMAN :—It is moved and seconded that three or five delegates be appointed to represent the American Institute of Electrical Engineers at the Electrical Congress of the Frankfort Exposition and that the Chair make the appointments at its convenience.

[The motion was adopted, and Mr. Carl Hering, Prof. Richard O. Heinrich, Dr. Edward L. Nichols, Mr. Ludwig Gutmann, and Mr. Nikola Tesla were subsequently appointed delegates.]

THE CHAIRMAN :—We have left over from yesterday's programme a paper of Mr. Frank J. Sprague, on the Rapid Transit problem. I understand that Mr. Sprague is not yet here, but that he will be present in the course of the morning.

The next paper to be read is one upon an Alternate Current Potentiometer, by Prof. Geo S. Moler, of Cornell University. I believe Dr. Nichols has the paper in charge.

DR. NICHOLS :—Prof. Moler is unable to be here and I am authorized to read the paper for him with the permission of the Institute.

Dr. Nichols then read the following paper:

A paper read at the General Meeting of the American Institute of Electrical Engineers, New York, May 21st, 1891. Vice-President Lockwood in the chair.

AN ALTERNATE CURRENT POTENTIOMETER.

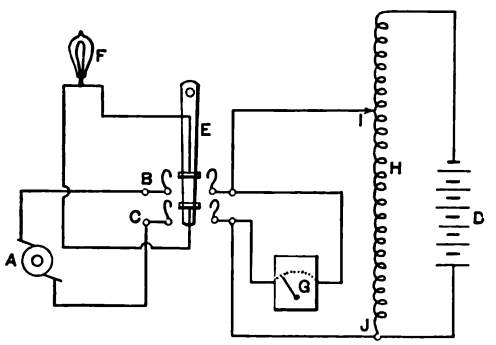
BY GEORGE S. MOLER.

This device is designed for the purpose of determining what values of the potentials of alternating currents will produce given effects, and is intended to be used mainly in calibrating alternate current voltmeters. It may also be used as a voltmeter, applying it directly to the alternate current to be measured. By means of this apparatus we may determine whether the instrument which is being calibrated changes its reading for other rates of alternations. This is shown by comparing the calibration curves obtained by using widely different rates of alternations.

The instrument, briefly described, is as follows: An incandescent lamp is so arranged and connected that it can be switched back and forth from the terminals of an alternating current line to those of a direct current line, which can be altered in voltage till the lamp glows with the same intensity when fed by either line. Then, while switched into the direct current line, the voltage of that line is measured by a direct current standard voltmeter or potential instrument, and the voltage so read is equivalent to that of the alternating current, which produces the same effect in the lamp. The switch is so constructed that it snaps back and forth, making the change of connection so quickly that the slightest change of intensity of the light can be easily detected. The lamp has a blackened hood with an opening in one side through which it is viewed while the sliding resistance is being adjusted. This enables the observer to make a much closer setting than otherwise. A very close setting may also be made by observing the slight reflection of the light from the blackened surface of the hood.

The lamp and switch are arranged and connected in the following manner: Referring to the diagram, A is the alternate current dynamo, or line, of which we wish to determine the effective difference of potential in volts between its two terminals, B and C; D is a storage battery or direct current dynamo of higher voltage than the alternate current to be measured. A switch, E, made to snap back and forth by means of a stiff spring, changes the incandescent lamp, F, from the alternating current to the direct current and *vice versa.* By connecting the two extremities of a resistance, H, to the battery, D, and sliding the contact point, I, such a place can be found that the intensity of illumination will be the same for both currents.

After the contact, I, is adjusted, the standard voltmeter, G, is read while the direct current is flowing through the lamp, and



ALTERNATE CURRENT POTENTIOMETER.

this voltage is taken as the equivalent voltage of the alternate current.

A series of lamps of different voltages affords a greater range to the instrument than could be obtained with one lamp only. These may all be connected to the switch and, beginning at the one of highest voltage, one after another may be tried till the best one is found.

For higher voltages than the highest voltage lamp will stand, a number of lamps may be used in series, and the fall of potential for all, or for each individual lamp, can be measured by means of its voltmeter.

The resistance, H, must necessarily be great enough in amount so that only a small current will be allowed to flow through it, so that the consumption of energy will be small.

Instead of having the switch and lamp connected in a shunt line to the resistance H, it may be connected in series with H, which must then be arranged so that it can be varied in amount. If a source of constant potential large enough in amount is available, the apparatus may be used for a voltmeter for direct or alternating currents. To use it in this manner, we calibrate the apparatus to find the potentials at the terminals of the lamp for different positions of the contact points I and J, and construct a curve, using the positions of I and J as abcissæ and the differences of potential of those points as ordinates. Then, when using the apparatus as a voltmeter, we read the values from this curve instead of from a voltmeter connected to the switch.

The apparatus was tested in the following manner to determine its sensitiveness, and to what extent its indications can be relied upon: The terminals B and C were connected to the same resistance H by two other contact points, and these connections were changed several times to include different amounts of that resistance. Each time this was done the contact point I was moved along till the lamp seemed to indicate the same candle power for both positions of the switch. The readings of potential for both positions were then made upon a standard potential galvanometer which has a scale graduated to millimetres, and upon which a double deflection of one cm. means one volt. Upon this instrument readings could be estimated to one tenth of a mm., giving volts to the second decimal place. The accompanying table of results were the first part of the first set taken in this manner and were consecutive. They were taken without any preliminary practice. From this it will be evident that the adjustment of the light can be made to within one-tenth of a volt.

LEFT CONTACT. VOLTS.	RIGHT CONTACT. VOLTS.	DIFF.
29.95	29.85	+.1
29.62	29.60	+.02
33.07	33.11	—.04
37.56	37.55	+.01
42.52	42.60	—.08
60.95	60.90	+.05
74.68	74.72	—.04

Physical Laboratory of Cornell University, May 9th, 1891.

Discussion.[1]

The Chairman:—You have read the paper and you have heard Dr. Nichol's abstract of it. A full and free discussion is in order. We shall be glad to hear from as many members as may have studied the subject, and others who are in search of information.

Mr. G. W. Blodgett:—I would like to ask if it makes any difference what kind of a lamp is used or whether one must use some particular lamp for a particular instrument.

Dr. Nichols:—The lamp I think is a matter of no consequence, only in order to get the best result the lamp should be more than red hot. You will find the sensitiveness to increase as the brightness of the lamp increases up to a certain point. It is best to have a lamp the voltage of which is about that which is to be measured. Through a wide range of intensities in a lamp, the accuracy is not materially diminished. Any constant current voltmeter can be used for reading the voltage on that side of the switch. In order that your result may be correct you should have a constant current voltmeter which gives correct indications.

Prof. Thomson:—As to the degree of accuracy attainable in this way if I remember correctly, some tests were undertaken some two or three years ago by us to determine how much variation was possible in a lamp without affecting the apparent illumination or what was the smallest increase or decrease of voltage suddenly brought on that would show in a lamp, and I think it was about 1-10 of a volt. It could be detected by watching the lamp very closely—simply cutting in and out a resistance in circuit with the lamp quickly we could detect about 1-10 of a volt, which of course would confirm the method as accurate to that extent.

Mr. Weston:—I was not present when Prof. Nichols read the paper but as I understand the matter it is proposed to use an incandescent lamp as an indicator to measure the potential difference of an alternating current circuit That is, we are to place an incandescent lamp on the alternating current circuit, the potential difference of which is to be determined and note the degree of brilliancy of the lamp, we then transfer the lamp to a direct current circuit and adjust the current passing until the lamp has the same brilliancy as on the alternating current circuit, we then measure the potential at the terminals of the lamp. The result expresses the difference of potential on the alternating current circuit. Is that the proposition?

Dr. Nichols:—Yes.

Mr. Weston:—That being the case, I can say from experience that the method is capable of yielding very accurate results. Some three years ago I suggested the same method to one of my

1. By Messrs. Blodgett, Nichols, Thomson, Weston, Geo. Forbes, John Waring and Lockwood.

assistants, and it was used by him in solving a somewhat difficult problem in connection with an alternating current voltmeter of great sensibility and portability. I think the method was used about three years ago.

To determine the brilliancy of the lamp, a photometer was used. The degree of accuracy is somewhat dependent upon the lamp used, or its degree of brilliancy. The greater the brilliancy, within certain limits, the greater the accuracy of the method. The method is undoubtedly capable of yielding very accurate results.

Dr. Nichols:—I think that it will be found that there is a limit to the brilliancy of the lamp which you can use in practice, on account of the eye. If you have too bright a light, the eye is not susceptible to small changes. Probably there is a maximum brightness which should be used, in order to give the maximum sensitiveness of the method. The change of the lamp will, of course, increase as you increase the intensity of the lamp. If you go to a point where the lamp dazzles the eye, the eye by its decreasing sensitiveness will overcome this. Our experiments thus far, go to confirm Mr. Weston's statement that the lamp should be burned at quite a high temperature.

Prof. George Forbes:—I would suggest that there is a very simple way of enormously increasing the sensitiveness of this method. As I understand it, the way in which you use it at present is to look at the lamp while the continuous current is serving it, and then switch it over to the alternating current, and then judge which is the brightest, doing it rapidly backward and forward. It seems to me that that method would be enormously more sensitive if you applied two lamps, and used them in a properly constructed photometer with a switch arrangement, which enables you to connect either of them, with the alternating or continuous current. Thus you would adjust the potential of the continuous current until you got very near equality, but you would find that one of the lamps was always a little brighter than the other, owing to the inequality of the lamps. You would then alter the distance of the lamps from the photometric screen slightly, so that on reversing the current you would get an absolute equality, and then you would be able to arrive at a much closer comparison, and it would not matter if there was a very slight difference in the illuminating power of the two lamps with different voltages. I feel perfectly sure that this would increase the sensitiveness of the method about ten fold.

The Chairman:—I would like to ask those gentlemen who have made this matter a subject of study, whether the eye doesn't eventually tire in watching the indications of the incandescent lamps. I know the ear tires in performing kindred operations for the telephone. I should imagine the eye would tire a little sooner than the ear, if anything.

Prof. Nichols:—I think there is no question about that. Any

one who works with a photometer becomes very weary. I do not know, however, whether the eye tires more quickly in watching an incandescent lamp and its changing, provided it be not too brilliant, than in reading closely a scale. I think reading a scale rapidly will tire the eye almost as soon as the other operation.

MR. JOHN WARING:—I think that the eye does tire, if you watch an incandescent lamp that is up to its normal candle power At the Perkins lamp factory we occasionally get a lamp with a bad spot in the carbon, which comes through the pump room without being noticed. To pick those out we are using at the present time a photographic lens, which throws the image of the carbon on a screen right in front of the person who is using the photometer. The light in the photometer is so mild that the eye doesn't tire at all. With a single glance at the screen, the observer can readily discover slight differences in the carbon, while the person who is hanging the lamps will have his eyes so fatigued with the bright light that he cannot see these slight defects at all, and I should think that in any work where a person tried to look at a lamp of normal candle power, the eye would soon become fatigued.

MR. WESTON:—For several years I had occasion to make numerous photometric tests of incandescent lamps, and also had several assistants engaged in this work. I never observed any fatigue of the eyes, which deprived them of their power to distinguish slight changes in the illuminating power of the lamps, nor did any of my assistants ever refer to such fatigue. Of course if the eyes are suddenly and frequently exposed to very bright light and, then to the feeble light on the photometric screen, I should expect them to fail to observe slight changes in illuminating power. I am inclined to think that when fairly used, the eyes may be depended upon to distinguish very minute changes in the illuminating power of lamps, provided the color of the light from the standard is nearly the same as that of the lamp to be measured. I presume that Prof. Thomson has had much experience with the photometer, in measuring the candle power of incandescent lamps. and has probably noticed that the eye does not get tired, when used properly. I am inclined to the belief that a skillful observer will make just as accurate determinations in the latter part of a day's work as the earlier, probably even more accurate.

PROF. THOMSON:—My own personal opinion about the matter would be that the eye as an instrument is a remarkably efficient one, and not subject to tiring, but that it really is the attention that tires. If you look for an action that is slight and there is a concentrated attention on such slight change, I think it is probably the brain molecule back of the eye that becomes tired. Perhaps the eye is capable of keeping up the record of images almost indefinitely, but back of that the capability depends almost

altogether on the strain on the nervous structure—how slight an effect is to be looked for and how often you must do it in a given space of time.

THE CHAIRMAN:—I confess for my part that I think we are too much accustomed to speak of the functions of the eye and the ear and the other organs of sensation when we should rather regard them as simply the agencies whereby impressions are conveyed to the brain. Of course then it is the attention which we have to fix upon these different impressions and these different things which we are studying at any one time which fatigues the brain. When we have to fix our attention constantly upon one thing, no matter what, since an effort is required to fix it there, I imagine that portion of the brain becomes a little fatigued, and after a rest it is better capable of receiving that impression in a fresh manner.

If there are no further remarks, we will proceed to the next paper, "Considerations Which Should Govern the Selection of a Rapid Transit System in New York City."

Mr. Sprague then read the following paper:

A paper read at the General Meeting of the American Institute of Electrical Engineers, New York, May 21st, 1891, Vice-President Lockwood in the chair.

CONSIDERATIONS WHICH SHOULD GOVERN THE SELECTION OF A RAPID TRANSIT SYSTEM.

BY FRANK J. SPRAGUE.

It has been but a short time since the Legislature of New York, in response to public demands as voiced by the press, in petitions, and through the personal efforts of the representatives of this city, enacted a rapid transit law to create a Board of Commissioners with large powers for the purpose, of determining rapid transit routes for this city.

During the past three months the Commission appointed under this law has held numerous public meetings, at which various systems have been proposed. These meetings are now practically at an end, and it is currently reported that a portion at least of the rapid transit routes have been determined. Since this subject of transit is one which has a most important bearing upon present and future New York, and is one in which the public has vital interest, it is eminently proper that so much of the Commission's deliberations as have through its chairman been reported should be the subject of public comment and critical analysis.

The public is naturally impatient in demanding results, and has been unduly free in criticisms directed against the members of the Commission, often assigning to them motives and proposed actions based upon their known or assumed business associations, which criticisms are undoubtedly, in many instances at least, wholly unjust. On one subject, however, that public is particularly sensitive. It sees the daily encroachments made by the Manhattan system, apparently under cover of the law but in

direct violation of its express provisions, and has no reason to be other than suspicious of the acts and intents of that corporation.

Probably there is no more flagrant example of this than the present work being carried on from 59th street up, on the Harlem division Already the cross girders connecting the two track structures for nearly the entire section from 72d street, to above 155th street, have been replaced by heavy lattice girders on which are supported either lattice or plate longitudinal girders, for a third track. On a large portion of this section the third track has already been laid. The method adopted seems to be the construction of a switch connection of moderate length, the connection of which with the main track is eliminated as soon as another adjoining section is completed. The rapid transit law expressly forbids the laying of any third track except with the concurrence of every member of the Commission, and since no steps are taken to prevent this construction, and since it seems likely that the Manhattan company would not go to the expense of making this change without some assurance that it would have the necessary permission granted to it, or that it would not be interfered with, the public naturally ask, What is the influence possessed by this company? How far does it dominate the Rapid Transit Commission?

I will not undertake to answer these questions, but it should be remembered that this Commission is composed of five men whose selection at the time made met with general commendation; that they are each men of independent fortune; each is distinguished in certain independent business lines; and all are recognized as men of public spirit, capacity and probity. They are, moreover, armed with great authority, can be safely said to be conscious of the gravity of their decision,and intent on giving to it most serious and weighty consideration.

It is then fair to accord to them an honest desire to make a determination of this question of such character as will, both in the present and in the future, prove to be undeniably the best, reserving the right of criticism both of result and motives until after the announcement of a decision which should not be unduly hurried.

Before considering the current reports of the Commission's deliberations, a general review of the subject may be advisable. There are two classes of people who are concerned in the final

decision made, the public and the capitalists. In order to satisfy as far as possible the demands of both, some of which are common, it is well to examine them separately.

To accomodate the public, it is necessary to:

1st. Supply present demands by following as nearly as possible the most important existing arteries of travel. An express train on the banks of the Hudson would be of little use to Broadway.

2nd. Provide for those future lines of traffic which, other things being equal, will most naturally be formed.

3rd. Make the most numerous and convenient connections for existing and projected local and suburban transit lines in directions other than those followed by the main system.

4th. Interfere least with changes in present buildings, or the erection of future ones. In no city in the world is this change more pronounced or continuous.

5th. Cause the least disturbance to, or weakness of foundations, present or future, and unnecessary displacement of service pipes, whether steam, gas, electric, air or sewer.

6th. Avoid disfigurements of streets and unnecessary appropriation of them.

7th. Inflict the least damage to property, either during construction or afterwards in operation.

8th. Provide the highest express and way speeds, each on independent tracks.

9th. Attain this last with the least liability of derailment or other accident.

10th. Provide the easiest access to stations, taking of all trains from one station, transfer from way to express trains and vice versa, from up to down trains, or to cross-town trains, or other intersecting lines.

11th. Finally, it should be built as quickly as possible, and should possess great flexibility.

To attract capital, the system:

1st. Should afford a reasonable and certain return on the money invested in the near, and a greater return in the remote future.

To do this, the road must attract passengers, which it will do by following the main arteries of travel and by affording increased speed, with increased comfort and safety.

2d. Must be built economically, and as cheaply as possible consistent with sound plans for future growth.

3d. The cost should be calculable with reasonable accuracy. This cost is comprised in a number of elements, as follows:

Preparation of Way, such as tearing down buildings, digging up streets, moving pipe systems, altering sewers, sinking of shafts, etc.

Actual Construction, such as building of viaducts, erecting superstructures, driving tunnels, laying road-beds.

Temporary Damage to, and blocking of public and private business and travel during construction.

Permanent Damage to Property, whether caused by proximity to road, noise, dirt or vibration, weakening of foundations, or interference with future buildings.

Building Space Taken Up, either by the roadway, moving trains, or necessary clearance above them.

Equipment of Rolling Stock, less with steam, greater with electricity.

4th. Should be such a construction as to make additional tracks possible, if future demands should warrant it, whether such tracks be parallel to, or intersect with, the main system.

5th. The process of construction should be hampered as little as possible by injunctions, or legal complications of any kind.

6th. The plan of construction should be such as can be most readily begun, most rapidly carried forward and most quickly enable the road to become a dividend earner.

7th. The franchise granted should be secure from unjust restriction and future interference.

8th. The raising of capital should not be made difficult by the granting of privileges for extension to existing corporations, of such a character as to make competition in the same field. An extension of the "elevated" up the Boulevard would have its influence on any other system proposed for the same route.

That the demands of both public and capitalists, as here outlined, should be as far as possible met, goes without saying.

Up to very recently, from the reports of the proceedings of the public hearings which have been held, and the statements which have been made from time to time in the daily press, the Commission seems to have:

Listened publicly and with patience to all plans of routes and construction submitted for their consideration, whether good, bad or indifferent.

Selected from these plans the limited number which, during the past few years, have more or less engaged public attention, and have at least the merit of financial backing, the record of operation elsewhere, or the promise in a more or less satisfactory degree of fulfilling the demands of a rapid transit system.

Examined privately some of the representatives of some of the leading systems.

Not publicly discussed the question of motive power, but are privately investigating it, and have not yet determined what it shall be.

Expressed serious doubts as to the availability of electricity as a motive power.

Determined that, unless electricity is used, no form of tunnel, shallow, individual or deep, shall be used.

Made a personal investigation of the topography of the city, with a view of determining its present needs and future lines of growth.

Determined to first lay out a West Side route.

Are attempting the coincident determination of motive power and route.

Are much concerned as to property damages.

Such, as I say, until very recently, seems to have been the attitude of the Commission.

About a week ago, however, when the public had received the impression that it had about reached a conclusion to at least a portion of its labors, application was made by the Manhattan company for extensions covering some 18 miles of street. The character of the proposed construction, the routes demanded, are now matters of record.

It is but natural that the delays in submitting a route, after the Chairman of the Commission had announced that it was about ready to do so, being followed by an elaborate presentation by the Manhattan company of the plans for its proposed extension, which plans have taken a long time to prepare, preceded as this presentation was by an explicit declaration on the part of the officers of that corporation, that it contemplated no more extensions, naturally caused a feeling of distrust and uneasiness, and was followed by suggestions of possible bad faith on the part of the Commission, and this is no less annoying and disturbing because of its inherent improbability.

Why this action of the Manhattan company excites this feeling is easily explained.

The portion of its scheme which most vitally concerns the city of New York, is the proposed extension of the system from 33d street up Broadway, through the Boulevard to 170th street, and its continuation. This is simply an iniquitous proposal—no other term can be applicable to it—and if granted by the Commission, no matter with what restrictions, it will prove a vital blow to all real rapid transit in New York City in the life of this generation.

Let us see what will be the result of such an extension. That it would be built by the Manhattan company, if granted the right, is undoubtedly true; that it would prove a profitable enterprise, is equally true; that it might be used for a repetition of the methods of financiering which characterized the consolidation of the elevated roads into the Manhattan system, need not be doubted; that it will enhance the value of the property of that corporation needs no argument, but—

What will it do to satisfy the needs of those who to-day use the elevated railroads?

How will it affect the well-being of this city?

With what sacrifice of its best interests would these schemes be carried out.

It is hardly necessary to point out that public spirit, civic pride or any feeling in fact, save a sordid one, enters into the calculations of the Manhattan corporation. Its spirit is much the same as that which prevails most unfortunely in a large portion of our city government.

It is well to recollect that some of the same men who have earned lasting condemnation of the public by their advocacy, some time since, of an elevated steel structure on Broadway below 33d street, have again come to the front in support of the Broadway extension above 33d street.

The former scheme was for the time defeated. It has not been given up, but the basis of attack has been changed from lower to upper Broadway. Every capitalist knows the great value of a road of almost any character which can be allowed to follow this main artery of travel. Grant a franchise for the length of Broadway and the Boulevard, and there will be some interesting bidding for its control. The history of the Broadway surface road and the character of the street are in evidence. Hence, the ultimate object of this corporation is a line for the whole of Broadway.

How does it propose to get it? Defeated for a time in its assault on the lower portion, it has for months been getting the consent of property owners along the upper Boulevard, who, seeing a prospective rise in the value of their property, and careless of, or indifferent to, the real welfare of the city, and ignorant of all systems save those presented by the Manhattan company, have in many cases agreed to make no claim for damages in case an elevated structure is put up. Armed with these consents and practically assured of the consent of a city government noted for its venality, anxious to add a heavily paying extension to its system, with lower Broadway as an objective point, and intent on defeating the raising of capital for any other system if reported, it now seeks the consent of the Rapid Transit Commission for this extension.

Consents thus sought, and in many cases obtained, ought not to weigh as an argument in favor of this extension, because the needs of the West Side property owners can be equally well served by another method of construction. That there is an explicit or tacit understanding with some of the city authorities seems evidenced by the continuance in its present disgraceful condition of the upper Boulevard, a street which was laid out for, and can be made to be, one of the finest in the world.

Let us suppose for a moment that the Commission so far prostituted its powers as to grant this extension, and it is built—What would be the result? It should be borne in mind that the traffic of the present Harlem division of the elevated road above 59th Street, is mainly the growth of the past seven or eight years. It is a traffic created on account of the building which has taken place during that period The Boulevard extension would not seriously divert travel from the present Harlem division. It would add nearly double that mileage to the present elevated system, and would build up a traffic of its own. It is safe to say, that in less than a single decade the travel on this extension would be greater than now exists on the Harlem division, while the travel on this latter would be in no wise lessened.

There would follow a congestion of traffic at, and below, 33d street, which cannot be realized, and this is precisely the result wanted by the Manhattan company. It is impossible by any practical change in the Sixth avenue division to take care of this increased traffic, and the only outlet for it would be an extension of the road down Broadway.

Grant then the Boulevard extension for the Manhattan company, and within twelve months after the first train has run over that extension, the necessary steps will be taken by this same corporation for the extension of the Boulevard line down Broadway from 33d street, *and they will get it.* There is no avoiding this conclusion.

If present public opinion will permit an extension of this system above 33d street, on Broadway and the Boulevard, for the benefit of the Manhattan company and the West Side property owners, with the certain resulting congestion of traffic on that section of the present road which vitally concerns all citizens, then it will in sheer desperation be compelled to acquiesce in the subsequent demand.

If to-day an intelligent and respected commission, made up of men presumably fairly representing the intelligence and public spirit of this community, cannot be relied upon to prevent the appropriation of one of the finest boulevards possessed by any city, forming the extension of one of the greatest business streets in the world, and the debauch of all real public interests by saddling upon this city the extension demanded, then it is hopeless to expect any future commission to withstand the pressure which would be put upon it.

If then it would save lower Broadway, it must protect upper Broadway. The system it adopts must be independent of, not dependent upon, the Manhattan system. The rights of the West Side property owners must not be allowed to override those of the rest of the people in New York Their interests can be better served by some other system which would give real rapid transit and leave Broadway and the Boulevard uninjured. Nor should the property owners of this section be themselves content with a higher speed above 59th street, and a lower speed below 33rd.

The question is not, How can the promoters of the Manhattan system find a most remunerative opportunity for the investment of additional capital, or How can they obtain the largest return with the least expenditure? The public of New York is most concerned, possibly selfishly, with plans by which it can best serve its own interests, the real interests of a great city, one fast out-growing the iron-bound vision of the Manhattan company.

Fortunately it rests with any one man in this Commission to prevent any extension of the elevated system, and I think we

can safely assume that no such extension will be granted except such as is consistent with the best interests of this city. I would not take the position for one moment that no rights should be granted to the elevated railway other than they now possess. I would say let its service be improved as much as is possible, but such improvement of service should not be coupled with extensions which stand as barriers to a proper solution of this problem.

If there were no other way, nor any better one, to obtain rapid transit in this city than by the system set forth by the Manhattan company, I should say, "By all means, let the system be thus extended;" but every one of us knows that other methods of construction can be adopted which are free from a large number of the objections which are inherent to the elevated railway system, and knowing this, we are justified in doing what lies in our power to aid in the adoption of such other methods.

In investigating the various systems, it is pertinent to inquire:

When was each proposed?

How comprehensive is its character?

Which presents the most modern and progressive engineering?

Which will best satisfy the future as well as the present needs of New York city?

Which will cost least, be most quickly constructed, and hence most strongly appeal to capital?

To satisfactorily answer these queries, there are three things to be determined. They are:

Motive power.

Method of construction.

Routes.

In order that the Commission may have the greatest latitude of judgment, these should be determined in the order named—that is, the determination of motive power should precede that of construction, and the method of construction that of route.

Two systems of motive power only are under consideration—steam and electricity; the former a well-known agent, whose advantages and disadvantages are well known, the latter a newer application of force, one in which the layman is not so well versed, but one whose advantages and possibilities are well understood, and most thoroughly relied upon, by those who have made it their particular study.

The vital importance of letting the determination of motive

power precede that of construction and route, will be clearly illustrated by noting the limitations upon construction. There are five general plans which have been proposed.

(*a*) An elevated steel structure, similar in character to the Manhattan system, and occupying the public streets.

(*b*) The viaduct masonry construction similar to some of the London sections of English trunk line systems, the viaduct road of Berlin, or the Broad street section of the Pennsylvania Railroad, in Philadelphia, to run through the middle of building blocks and to span the streets with steel arches.

(*c*) A shallow tunnel, made by excavating the streets, removing or diverting the existing system of pipe services of all kinds, running the tracks in one or two tiers, either superposed or in different vertical planes, the street to be floored over with a steel girder structure. The inclosed track would be something like the Fourth Avenue Harlem Railroad tunnel, except that it would be entirely enclosed.

(*d*) An iron tubular closed tunnel construction, the tubes running at a depth of from twenty to forty feet below the surface, and built something like the North River Tunnel, but individualized like the Suburban and South London road constructed in London by the Greathead interests, and opened for regular service last November.

In this system, the various tracks would be generally independent, and the way and express routes likewise independent, save at common intersecting stations.

(*e*) A deep-rock, two-track tunnel, identical in character with every other rock or mining tunnel with which we are all familiar.

If electricity be adopted as a motive power, the Commission will be left untrammeled to determine which of these constructions is preferable.

If steam be adopted, considerations of construction are at once limited to plans (*a*) and (*b*), and even then there exists a limitation in the viaduct system not yet realized, to which I will soon refer.

I think it may be taken for granted—

First, that a West Side route should come as near Broadway as possible;

Second, that a four-track system is essential, because higher speed and greater capacity are required, and both way and express service must be provided.

An elevated structure necessitates the appropriation of streets already needed for other purposes. If for four tracks in one plane, such a system is impossible in most of our streets. It is hardly necessary to further discuss the objections to this system.

A viaduct construction requires for every foot of way, except at street crossings, the condemnation and purchase of property of great value in the business heart of the city, of an aggregate far exceeding the proper cost of road construction. It means the tearing down and re-erection of many buildings, the interruption and cessation of the business carried on, with all the necessary legal complications which will surely follow, thus increasing the cost of construction in an unknown but material way, and it would require extra provision of capital for the purchase and alteration of these buildings.

But, as I have intimated, there is a distinct limitation to this plan in the simple one of motive power. There are two plans of constructing such a viaduct, one by making a clean cut through the buildings, leaving the roadway uncovered, and the other by tunneling through the buildings. If the former plan is adopted, there would be thrown away a continuous strip of block space in the most valuable section of the city, from twenty five to fifty feet in width, according to whether the tracks are in one or two planes, and from fifty to three hundred feet in height. Although the value of this wasted space is well nigh incalculable, it is the only plan by which steam could be made available, because if the roadway is covered in the block spaces, and steam be used, the same objectionable features would exist which have given the Fourth Avenue Harlem Tunnel so unenviable a reputation, and the damage to property would be even more pronounced. If, however, the viaduct construction were adopted, economy would demand the tunnelling rather than the cutting-down method of getting through buildings. This consideration would then require the abandonment of steam and the adoption of electricity.

But suppose this suggested construction and motive power be adopted in the populous portion of the city, and the street crossings be left open; there would then be that most disagreeable alternation of light and darkness, or of a greater or less degree of light, which is another of the objectionable features of the Fourth Avenue Tunnel, and hence the crossings themselves must be enclosed, and with what result? The additional disfigurement of

the street may be here ignored, but there would be simply an elevated tunnel of the most costly character, supported on a masonry viaduct, and a system of most questionable financial promise.

In considering an elevated or viaduct structure, following the approximate route outlined, it is well to note what obstructions would be met in its course in the lower part of the city.

How, for instance, would such a system cross the Manhattan system at 34th, 53d and 65th streets? Would a grade crossing be advisable or permissible? Unquestionably not, for the danger of collisions and the delays and blocks would at once condemn it. If not a grade crossing, then which road would be uppermost? The Manhattan being already in possession, the new system must do the crossing and at an abnormal elevation. What would be the effect of the operation of such a road, upon the Metropolitan Opera House?

As regards stations, the elevated and viaduct plans require separate up and down platforms, and it would be interesting to inquire how, with the limitations which must exist on roads of this character, it is possible to serve way and express tracks on a common level, except by making them common at the way and express stations. The danger attending this practice must be apparent.

Returning again to the question of motive power, it is important to know what are the objections raised against the use of electricity. About the only one made publicly is that it has not yet been used on as large a scale as would be required in the proposed system. This reason seems ultra-conservative, and it is to be regretted that on a subject of such vital importance, there has not been made a more thorough public inquiry.

This same view was taken of the application of electricity to street railway propulsion only three years ago. There was scarcely an engineer, a street car builder, a street railway constructor, or a capitalist, who did not laugh at the claims which were made for it, and yet in this short time there has been a total revolution of feeling. There is not, probably, a street railway manager in the United States who does not thoroughly believe that electricity is the present and coming power for street railways, and who, if he were constructing a new road or re-equipping an old one, would not at once, or in the near future, adopt electricity, if he could get the necessary rights to do so; and it

should be borne in mind that the equipping of these roads has meant, in very many cases, the entire rebuilding of the track, the purchase of new cars, the building of costly stations, and, in short, the investment of a greater capital than had already been invested in the original plant, so that fifty million dollars is a small estimate of the sum which stands invested to-day in this one industry alone.

If, however, the Commission is still in doubt, then in order to get a more intimate knowledge of what electricity can do as a motive power, it would seem proper—

First—That a time be set apart for the public examination of electrical engineers and contractors.

Second—To require a private statement from electrical engineers, confidential if desired, as to any new developments not publicly known.

Third—To visit two or three of the prominent electrical establishments to see what has been, is being, and what will be, done in general electrical machine construction.

Fourth—To then state what duty would be demanded of electric motors, and what guarantees of performance would be required under forfeiture.

If something like this be not done, then I think it safe to say that the rejection of electric motive power will be because of the following reasons: Its novelty; allowing accidents incident to the early period of its development to outweigh in importance its demonstrated advantages; a conclusion that it is limited in capacity and development; and lack of information as to its past, present and promised future.

Such a rejection would be upon technical advice relied upon by the Commission.

This, it would seem, ought to be that of the Board of Engineers, not the judgment of any one man, because no one has sufficient experience to warrant such reliance upon his dictum in a matter of so grave import, on a subject to which so many able engineers are devoting themselves, and the development of which has enlisted so many millions of capital.

The motor equipment on, for example, the present London road is composed of one hundred horse power units. That required on the elevated railroads is one hundred and eighty-five horse power. Attach four motors of the London type to the same circuit instead of two, govern them, as is perfectly feasible,

by the same controlling device, and they will outwork any elevated road engine.

Increase the dimensions in these machines one-third only, and two of them will do the work of this engine. Duality may be objected to, but it should be borne in mind that duality of engines is absolutely necessary in a locomotive for successful operation. It is not essential in an electric motor equipment, although advisable for traction purposes. But for that matter double the dimensions of a London machine, and it alone, will outwork the two cylinders of the elevated locomotive.

Perhaps one of the most telling arguments in favor of electrical propulsion is the fact that in London another underground road to be operated by electricity, is about to be constructed on plans already approved by a parliamentary committee. This road is about six miles long and connects the West End with the city. It forms approximately a diameter of the oval described by the present steam underground railways. A 5,000 horse power station at the suburban end of the line will supply the current. The trains are to consist of six cars, seating about 340 persons, and will weigh, with the electric locomotives, 120 tons. The average speed is to be 14 miles an hour including all stops, with a maximum of about 25 miles an hour. Since this weight, power and seating capacity exceeds that now common on the elevated railroads, it is a somewhat practical answer to the objections made on this point.

There have been no demands for a two thousand horse power steam locomotive, but there is not a builder in this country, who, given suitable gauge of track and allowed dimensions of engine, would hesitate a moment to build a locomotive of that capacity.

So too with electric motors. Given the incentive, and before the first foot of roadway is ready on the proposed Rapid Transit System of New York, a two-hundred horse power electric equipment will be built which will, under forfeiture, outwork any elevated road engine for speed, pulling capacity, steadiness of work, time of operation, and continuous mileage.

This statement is made from personal knowledge, and with an individual willingness to assume the risk.

Break a single part of the many in a locomotive and it is helpless. Break many of certain parts in an electric motor and it will, although crippled, still operate. In short, the near future will make the electric motor the most reliable machine constructed for power purposes.

What is the experience of other large cities in this matter of rapid transit?

In London the same interest which built the Greathead tunnel has been granted parliamentary right for extensions, ordinarily most bitterly fought against, with an almost unexampled unanimity.

Paris is considering seriously the question of a great metropolitan underground electric system.

Boston has been advised after most careful investigation that the only proper system for the business portion of the city is an underground electric system.

As regards New York, while unnecessary delay is to be avoided, it is certain that no present delay is half so serious in its consequences as the adoption of an inferior plan by reason of immature consideration, lack of knowledge or information, or in response to popular clamor. The demands of to-day can be best served by plans to meet the future wants of New York as well, while the needs of the future cannot be properly provided for by planning for present limitations. In discussing the subject some time since in a communication addressed to the Commission which contained the gist of much I have already said in this paper, I made the following suggestions:

It is possible that the Commission, while still in doubt about the availability of electricity as a motive power, may feel it incumbent upon them to announce some decision with regard to at least one route. Would it not then be possible to submit two alternative propositions, the preferable one of which could be determined by public comment and criticism and the readiness with which a responsible construction company would bid for the necessary right and franchise?

First: Questioning the availability of electricity, select the best system which can be advised other than an underground system, that is, some elevated or viaduct structure. Specify the number of tracks, the route over which it shall run, and make steam the motive power.

Second: Accepting the statements which are made in favor of electricity, select the best underground system, which will answer as far as possible the demands which have been enumerated. establish the route, the number of tracks, and make use of electricity obligatory.

Third: Demand as regards

Construction: Earliest time of beginning, greatest freedom from interference, either with streets or existing systems of pipes, least damage to property, most rapid progress of work, earliest completion.

Results: Mean and maximum speeds, number and weight of trains operated, approximate cost of motive power per train mile under these conditions.

Guarantees under Forfeitures: Against construction damages to buildings or foundations, and against future damage claims.

Then let the public discuss the merits of these alternative propositions, and let a construction company or companies bid on these systems and on the franchise, accompanying such bid with a proffer of a certified check of sufficiently large amount, and then see which company will—

(*a*) Guarantee to build most quickly.

(*b*) Make the best offer for the franchise.

(*c*) Offer the highest guarantees, both as to progress, cost and satisfactory operation.

As I close this paper, there comes news of most reassuring character, for the Chairman of the Commission is reported in Tuesday's *Commercial Advertiser* and in Wednesday's morning papers, as stating in the most emphatic manner that the Commission is planning for future New York, and that it will under no consideration even consider the plans of the elevated railroad until after it has determined upon a route to satisfy the demands of that future.

This statement of the Chairman is most opportune, and will go far to allay the anxiety which has been honestly felt by those who await the result of the Commission's deliberation.

It is also stated, presumably on the same authority, that a system and West Side route from the Battery to the Harlem River has been definitely settled, part underground and part viaduct, and the announcement of this decision will be definitely made next Tuesday. If so, then it would seem that electricity has now the victory, and the particular method of construction and the route selected will form the subject of discussion.

Discussion.[1]

The Chairman:—The Institute has good reason, I conceive, to congratulate itself on being the recipient of such a thoughtful and thought inspiring paper, upon such a subject. Although the electric railway, rapid transit in general, and the railway motor, is perhaps not human nature's daily food, it is certainly no small element in carrying the people of great cities to where they can get their daily food, and in sometimes carrying their daily food to the people, and as such it is a subject that should have our most careful, and perhaps in some respects, our anxious thought. We are to be pitied also, as well as congratulated, that we have not had copies of this paper in order that we might read it as we go along, checking the speaker, as we have done in other cases, seeing whether he is reading his own paper straight. The trouble with such a paper is that any one can concur in the statements that are in it. While it deals mainly with local questions, these are such that the answers will be of general interest and will always be of value in considering the problem of rapid transit in other cities; and this, of course, is becoming the problem throughout the world. The conservatism of which Mr. Sprague spoke has been the lion in the path of every one who has sought to introduce any new agent in the progress of mankind, especially those which are the applications of electricity. No better example of that can be found than the table in the other room where the early struggles of Fulton in introducing the steamboat are recited. Discussion is now in order.

Mr. Mailloux:—Mr. Chairman, I can only state, as you have already stated, that we all concur generally and specifically in the statements and conclusions made by Mr. Sprague in the able paper he has read, and further that while I am deeply interested as a citizen, in the development of schemes for rapid transit in New York, professionally I am interested in rapid transit on a smaller scale. I am content to confine myself for the present with the running of street cars of smaller sizes and horse powers, and I am willing to leave the treatment of this more gigantic problem to Mr Sprague. I not only have my hands more than full with the consideration of the transit problem from another aspect—for surface work—but I think that the larger problem does not need very much coaching when it is in the hands of Mr. Sprague. I do not think that I could give him any points that he does not know better than I, having as he has, given that subject so much thought within the last few months. Possibly there is no one more competent than he to speak upon it authoritatively, and I doubt if we can add anything to the discussion that would be of value. When the time comes, after we shall have made some progress with the Commission, so to speak, in having obtained from them the assent to the introduction of electricity as a factor

1. By Messrs Mailloux and Forbes.

in municipal rapid transit, then of course the engineering details will come up for discussion, and in that, whenever it shall happen to come before the Institute, all of us who have had experience with electric traction may be pleased and interested to say a word; but as long as it is only to be discussed from a general, broad engineering standpoint, from a commercial aspect particularly, I do not think that anybody could make a more masterly presentation of all the factors involved in the case and a more comprehensive analysis of the conditions and difficulties of the problem than Mr. Sprague has done.

THE CHAIRMAN:—We have every reason to believe that Professor Forbes can tell us something we do not know about the South London road. We would be very glad indeed to hear anything that he might care to tell us.

PROF. GEORGE FORBES:—I know that there is always a general feeling over here that progress in electrical invention is retarded by the conservative spirit in Europe, and that we are slow in taking up things, and that you are very much further ahead than we in all electrical development. I think there is no one who is more ready in many cases to acknowledge that than myself, and frequently after my visits to this country when I have gone back I have been looked upon as a renegade for turning against our English modes of doing business, and for not supporting them, and for saying that you had been doing better in America than we were doing there. England has not in the past taken quite the position with electricity that she did with steam. She has not gone ahead and taken the lead in the world as she did with steam; neither in electric lighting nor in street railways. The reasons, however, for both of these are perfectly definite. Electric lighting was retarded in England owing to the defective legislation which was introduced and street railways would be working under different conditions in England from what they are here if it were possible to permit the overhead wires which we see in Boston and other cities of this country. If they could be permitted in London or other towns, we would be, I venture to say, not far behind you in the progress of street railways in England, but there are such objections to it that that is impossible, and if you, on the other hand, were deprived of the advantages of the overhead wire system for street railways you would be, I venture to say, as far back as we are at the present moment. Now, however, we are coming to a new stage of electrical progress and that is not of street railways but of real rapid transit railways, and there, I venture to say, that we have made a start ahead of the United States; that in the development of the South London Railway, which has been alluded to by the Chairman, we have made a step of very considerable magnitude—not a step of considerable engineering magnitude, but a step of considerable magnitude in overcoming the inertia of the rest of the world; in leading capitalists, municipal authorities and the public generally

to recognize that traction on a large scale is an accomplished problem. The development that has already proceeded since the completion of the South London Railway in bringing before Parliament the West Railway crossing the underground railway system, which Mr. Sprague has alluded to, and the other electric railway down the length of Oxford street and Holborn many miles in length, is an indication that it has been realized by all those interested on the other side that this is an accomplished problem. I have not the slightest doubt that the progress here will be very great indeed. But I did just wish to say a word or two in favor of what we are doing on the other side. So that, although I have always been ready to speak highly of the progress you are making here, I am not going to go back on my own country, and say that we do not do good things when we are really doing them. In the course of my frequent visits to New York I have taken the utmost interest in the question that is now before us—the question of rapid transit here, and I have discussed with the various persons interested in it the different methods which are proposed to be adopted, and I have, I confess, been considerably disappointed by the first words in Mr. Sprague's address this morning, though I most thoroughly sympathize with the reasons of his remarks. It is quite evident that on an occasion like this, it cannot be expected that consulting engineers should go into the whole development and details of the plan which they would propose if they were consulted with regard to the best system to be adopted in rapid transit in New York. Their plans are too much undeveloped and unsecured, and it would not be fair to expect them to discuss in a meeting like this the whole details of the plans that they would propose. But Mr. Sprague has given us a general idea of the problem as it exists now, and as the public are studying it, and he has given us the most lucid account which I could conceive, of the present position of the question of rapid transit, considered generally. I presume every one who has listened to his paper, in all probability thinks that electricity is the only means which will be adopted. I will now say, however. a few words as to my own experience of the South London Railway, so far as it seems to bear upon the problem here. The system is working with absolute perfection at the present moment. At the beginning of its running there were some little troubles, chiefly mechanical. There were no electrical troubles connected with the railway after they once got the proper engine down to work. At first an engine not quite powerful enough for all they wished to accomplish was put down But that was soon replaced by a more powerful one. The only troubles which were shown at first were due to want of balance in the cars and occasional derailing. That has been completely obviated, and now its service is as perfect as could be wished. The comfort of traveling on this line is worth speaking about. I have noticed in talking with New York men that their idea is that an

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underground railway is necessarily objectionable. If those persons speak from their experience of the underground steam railway—the District and Metropolitan railways—I must say it is the most perfect good sense. Traveling on that system is simply atrocious, especially in warm weather. But on the South London Railway the comfort is perfect, the temperature is uniform summer and winter. There are not the fumes that are in the underground railway in London, and there is no vitiated atmosphere in any way whatever. The tunnel is air-tight and water-tight. No sewage could possibly filtrate into it. It is at a distance below all sewers and sewer contamination, and the ventilation secured by having two tunnels, the one for going and the other for returning, is absolutely perfect. The arrangements for large elevators—hydraulic elevators at the stations for carrying people to the surface of the ground—are also perfect. The only point which strikes one as a little curious is that besides the electric system for propulsion and the hydraulic system for the elevators, we also have the compressed air system for the brake, bottles of compressed air being carried on the cars. It may be that an electric system of brakes might be harmonious with the other parts of the system, but I may assure you here that the comfort of traveling on that railway is perfect and could not be improved upon. The little noise there is, is not very objectionable. Nor would it be in a town like this, where we are accustomed to considerable noises, without raising objection to them.

My own opinion is certainly in support of the view which Mr. Sprague has initiated, that the solution for rapid transit in New York is by tunneling and electric traction. As to the particular method of electric traction, I will not say anything about that at present. It is a mistake to suppose that there is only one system of electric traction which could be put on—that heavy locomotives pulling the trains is the only way by which the problem could be solved. But out of the many ways which may be proposed, I feel sure that the engineering ability which will be called in to discuss the question and to arrange the plans—out of that I am sure that the outcome will be highly satisfactory. I have very great confidence that if these causes, to which Mr. Sprague has alluded, outside the engineering, be properly dealt with, that the problem of rapid transit in New York will before many years be solved in the way which electrical engineers would wish it to be solved. [Applause.]

THE CHAIRMAN:—Before leaving the subject I might say that when I came home from Europe a couple of years ago, I spoke so warmly of some construction I had seen there, that there was some evidence that I was to be treated as a social pariah, so that I can sympathize with Mr. Forbes.

If there is no further discussion on this point, we will now be very glad to hear the paper of Mr. George W. Walker, on "Electric Meters."

Mr. Walker read the following paper:

A paper read at the General Meeting of the American Institute of Electrical Engineers, New York, May 21st, 1891. Vice-President Lockwood in the chair.

ELECTRIC METERS.

BY GEO. W. WALKER.

The time has come for the central station companies to realize that they must supply the electric current in a more thoroughly commercial manner—that is, it must be sold as other commodities are sold, by measure, weight or its equivalent. To convince, however, an already skeptical public that the electric current can be measured with as great if not a greater degree of accuracy than the established and accepted methods of measuring gas, water and steam, is no small undertaking. The fact remains, however, that we have to-day a number of electric meters which will measure the electric current more accurately and thoroughly than instruments adopted for similar purposes in other branches of business.

We have, it is true, one great question which we cannot answer, and that is what the electric current really is. We explain this, however, by saying we can measure its effects, which are reflected either in light, heat or power. Fortunately for us, we have adopted units which are universal; the coulomb, ampere and ohm represent values practically the same throughout the entire world. Therefore we start out with fixed rules which must be recognized as well by the old world as the new. Compare this fact with the different weights and measures adopted by various nations and countries, and see what universal dissimilarity exists. Imagine for a moment what consternation would exist among the great dry goods houses if it were possible to instantly transfer all our yard sticks to France and their metre sticks to this country; the result would be to momentarily paralyze their business. We can, however, place our measuring instruments on

any circuit, and the result remains the same and is read with the same degree of certainty and accuracy by all nations.

As gas was the follower of the tallow dip, so is electricity the successor to gas, and one of the legacies inherited by us is the much-abused and anathematized gas meter. The peculiarities of this somewhat venerable object are numerous, sometimes amusing, many times annoying and expensive, as our personal experience will prove. The original gas meter was a very crude affair, indeed, and would insist on freezing or drying up at the wrong time. Probably many of us could tell amusing experiences which have befallen us in our endeavor to make it work. Its progress to its present state of perfection was very slow; in fact, for almost if not quite twenty years after it was first introduced, no improvements were made upon it. Much as we blame and question its arithmetic, it is undoubtedly a very reliable instrument. It has, however, by comparison, caused the ever suspecting public to look with suspicion upon the electric meter, even though they "see the wheels go 'round."

In presenting this subject to you, it may be interesting to know that although there are but a very limited variety of electric meters now on the market, this number does not in any sense represent the thought and time that has been given to this particular branch of the electric field. A perusal of the United States Patent Office reports show that up to date about 185 patents have been granted on electric meters alone. The first patent granted by our government on electric meters was No. 132,569, dated October 29th, 1872, and issued to S. Gardiner, Jr., of New York City. The mechanism of this meter consists of an electro-magnet, having its armature connected to a releasing device of a clock movement, the dials of said movement indicating the time during which an electrical current is in action.

No further patents were issued until the year 1878, during which year a patent was granted to W. E. Sawyer, also of New York City. The same month, November 26th, a patent was issued to J. Billings Fuller, of Brooklyn, N. Y., on what was perhaps the first alternating meter invented. His invention is embodied in one claim which reads as follows: "An electric lighting system in which the light is produced by alternating currents, an electro-magnet placed in the main or induced current circuit, or in a shunt therewith connected, and having a polarized armature, the vibrations of which actuate a train of registering wheels, substantially as and for the purpose specified."

The patents granted to Sawyer and Fuller were the only ones issued in 1878. One patent was granted in 1879; three in 1880; seven in 1881; seven in 1882; eight in 1883; nine in 1884; five in 1885; twelve in 1886; thirteen in 1887; twenty-two in 1888; thirty-six in 1889; and about sixty during the year 1890 and up to the present time. You see, therefore, that considerable time, brains and money have been expended in the direction of perfecting the electric meter. A careful examination of the 185 patents issued, reveal many ingenious devices for the measurement of the electric current. But in most cases the expense of manufacture, the likelihood to derangement and the cost of operation kept these meters out of the race, from a practical standpoint. It would be impossible in the time allowed for this paper to describe a great many meters. I presume you will be interested only in those which have been placed prominently and in a commercial manner before the American public. I will take them up according to their seniority and describe them as briefly as possible.

The first to consider is the

Edison Electrolytic Meter.

The fundamental patent taken out by Mr. Edison on his chemical meter is No. 251,545, dated December 27th, 1881. In the patent, an electro-magnet is claimed as part of the meter, the principal claim, however, is in the use of an electrolytic cell placed in a shunt circuit, the resistance being so proportioned that a definite fraction of the current passes through the cell. The chemical action of this meter when in use, consists in depositing metal from one electrode to another, the rate of deposit being regulated by the amount of current passing through the meter. The electrical energy consumed is measured by ascertaining the increase of weight of the electrode which receives the deposit. Subsequent to the issue of this patent, he devised various modifications of this meter, introducing recording devices, as in Patent No. 304,082, which provides for the use of two electrolytic cells, one plate of each being suspended from the beam of a sensitive balance, the circuit being so arranged that one cell is in circuit at a time. The direction of current in that cell is such that the electrolytic action in the same will throw the balance out of equilibrium, causing the beam to kick and by that action throw the current to the other cell and register one on the counter. Various other devices were patented and experimented upon by Mr.

oscillate synchronously and the dial train is at rest. As soon, however, as the current passes through the large coil, the time of oscillation of the magnetic pendulum is affected, so that it swings faster as the current passing through the coil increases, the dial train registering the oscillation of the magnetic pendulum. In order to take a reading of the current consumed, it is necessary to multiply the degrees marked on the dial by the constant, which is determined for each meter individually. The clocks are supposed to run thirty days with one winding. This meter has been introduced quite extensively in Berlin. Judging from its general construction, it should give very accurate results, if operated under favorable conditions. Its size and cost, however, have prevented any general adoption of it in this country. I understand its representative in America has concluded to give up the fight and return to Berlin. This, I am told, was hastened somewhat by the passage of the McKinley bill, which imposed such heavy duties on their importation as to make it impossible to compete successfully with the other meters in the market.

The Thomson Watt-Meter.

Prof. Elihu Thomson has probably been the most prolific inventor of electric meters, as the Patent Office reports will prove. Previous to bringing out his watt-meter, Prof. Thomson's efforts were largely directed to perfecting what might be termed vapor meters. The general principle of this character of meter is the evaporation by heating effect of fluids, such as alcohol, ether and water, such fluids being confined in sealed vessels or chambers. Each meter provides for the use of two or more glass chambers, such chambers being connected by a suitable glass tube, and then delicately poised. By the heating effect of the passing electric current a change in the weight of one or the other of the vessels is produced, which disturbs their equilibrium, and by a suitable arrangement counts their oscillations upon a dial train. Prof. Thomson has devised many interesting meters operating on this principle, but, unfortunately, they did not appeal to the commercial public in a favorable manner. His most recent invention, and the one which has been adopted by the Thomson-Houston company, is what is known as the Thomson watt-meter. This meter is of the motor type and consists of a set of coils wound on a hollow frame, forming what might be termed a Siemens armature of polygonal outline, without any iron core.

Passing through this armature is a vertical shaft which rests in jewelled bearings at the top and bottom. The coils of the armature are connected to a commutator carried on the shaft near its upper bearing; two light springs bear upon this commutator and constitute the brushes. The coils of the armature are wound with fine wire and have in circuit with same, resistance which is wound according to the voltage required. The armature and resistance are usually connected in shunt across the mains. The armature has, surrounding it, a set of fixed coils, forming the field magnets, through which the current to be measured passes. Attached to the shaft which passes through the armature, is a gear which engages with a train of dials or counters for the purpose of recording the revolutions of the armature and, consequently, the amount of current consumed The means provided to regulate the speed of the armature consists of a copper disc, which is also attached to the armature shaft. This disc rotates between the poles of a number of permanent magnets and is retarded by their influence. In this case, as in other meters of this type, the difficulty of securing proper speed for light load and regulation on heavy load, is the great question to overcome.

The claim that this is an absolute watt-meter, I fail to see the justice of, inasmuch as the armature and resistance must be specially wound for any given voltage For example, a meter wound for the 110 volt circuit will not operate correctly on a 60 or 75 volt circuit, and vice versa. In fact, it is absolutely necessary to correctly wind the armature and resistance for the given voltage of the circuit upon which the meter is to be used.

The tendency of this meter to continue to operate after all lamps have been turned of has, it is claimed, been in a measure overcome by the use of brass instead of iron in the frame of the meter. However, the permanent magnets surrounding the copper disc seem to give out lines of force creating a field for the armature, which is usually in shunt across the mains, and in case the armature and resistance are not adapted to the voltage of the circuit, the meter will continue to operate for or against the consumer.

The principle embodied in this meter is not new, and it is only fair to add that as far back as the years 1883 and 1884 patents Nos. 233,547 and 304,881 were taken out by Mr. Edward Weston, on a meter of similar construction. The claims of Mr. Weston's patent provide for the use of a motor in which the armature

is connected across the mains and a suitable device for regulating the speed of same, also a dial train to record the number of revolutions made by the armature, representing the amount of current consumed. Mr. Weston's method of controlling the speed of the armature provided for the moving of one of the field magnets to and from the armature. The principles embodied in these two meters seem very similar.

The meters which have thus far been described, all provide for measuring the amount of current consumed in a culminate manner, either by a series of dials or by ascertaining the amount of metal deposited on an electrode. Therefore, any question as to the correctness of the result attained cannot be explained in detail. As I stated before, the gas meter is our legacy, and strange to say, the efforts thus far made by inventors have been to produce a meter resembling the gas meter as much as possible, both in its appearance and operation. The meter which I will next describe is one with which I am the most familiar, and which differs in its operation from any thus far brought to your attention.

The Walker Meter.

It has been usual, in testing any electric meter, to place in the circuit with same, an ammeter for the purpose of proving the correctness of the meter's operation. Assuming this hypothesis to be correct, we have in the Walker meter the element of a test instrument, inasmuch as the meter consists principally of an ammeter which measures all the current passing, and records by photography the various positions occupied by the ammeter needle. The use of photography in this case is to eliminate the element of friction in the meter. When I use the word photography, I do not wish to imply that there are lenses or any of the necessary devices pertaining to the photographic art, to contend with. The apparatus is simply so arranged that the ammeter needle point is represented by a speck of light, and as the needle moves its position, this point of light traces an image on the sensitive paper which is immediately behind the same. In other words, we are practically photographing the position of the ammeter needle during such time as it is being affected by the passing current, and the result is a continuous line marked upon the sensitized paper, representing the fluctuations of the ammeter needle

In detail, the meter consists of an ammeter coil or solenoid, in which an iron wire in the shape of a nearly complete circle, is

placed as a core. The same shaft to which the circle or core of the ammeter is attached, has also fixed thereto a light aluminium sector provided with a fine vertical slot; this slot representing the needle or pointer of the ammeter. Immediately behind this sector is a box which encloses a roll of sensitized photographic paper. The screen placed between the sector and the box has a horizontal slot before which the sensitized roll is made to pass. The vertical slot in the sector is so arranged as to come at right angles with the slot in the screen, so that in whatever position it may be it will always allow a point of light to pass from the lamp which is enclosed in the meter, onto the roll of sensitized paper.

Perforations in the screen, representing the calibration of the instrument, cause continuous horizontal lines to be printed on the sensitized paper. Another horizontal slot below these perforations is covered by a shutter which is opened every hour by the clock movement, and causes vertical lines to be printed across the sensitized paper, representing hours of service. The number of the meter is also printed every hour just above this hour line. Means have been provided for causing a continuous movement of the paper roll at a given speed, and this is effected by the use of a small clock movement which is wound automatically by the electro-magnet, placed in shunt circuit with the lamp in the meter. The clock movement remains stationary and the lamp goes out when no current is on the circuit, but both start up immediately upon the lighting of a single lamp. In this way there is impressed upon the moving paper roll a complete record in a detailed manner, of the current consumed in the circuit for a given time.

The roll of paper after a certain time, say one month, is taken from the box and developed in the usual manner, so as to fix the light impressions. It can then be measured up in the same way as an indicator card, by the use of the planimeter. The consumer is, therefore, at any time able to check the charges made, and the records being permanent can be used for future reference. It is evident that a large number of rolls can be developed in the same time required to handle a single one; in fact, the number is restricted only by the size of the vessel used. In the meter, as at present manufactured, each square inch on the diagram represents 16 ampere hours, therefore the total number of square inches measured by the planimeter need only be multiplied by sixteen to arrive at the sum total of the energy consumed. The loss in

operating this meter is confined to the energy required to burn one lamp in the meter which, at the candle power used, takes one-sixteenth of an ampere. This is the maximum loss, whether the current of one or a thousand lamps is being measured, the ratio of loss decreasing as the load increases.

The only care required in operating this meter is in placing the sensitized paper in the holder. In order to do this satisfactorily, a dark room should be built in a convenient portion of the station. A window of dark yellow glass in this room will provide plenty of light for the operator, but if more light is required this window can be covered with yellow post office paper and an incandescent lamp suspended before it. The sensitized paper in rolls of sufficient length for one month's use comes in sealed packages, and must only be opened and placed in the holder in the dark room. A boy can be instructed to collect the boxes, as skill is not required. As stated before, the meter number is printed on the diagram every hour, therefore it is not necessary to keep any record of the box itself.

It is evident that a meter which provides means to compute the current consumed in a detailed manner, has advantages which other meters do not possess, inasmuch as any question as to the correctness of bills rendered, can be satisfactorily explained by referring to the diagram or chart made by the meter which is in dispute.

The Patent Office reports show numerous devices for securing the record of the ammeter needle on a moving strip of paper, by the use of a siphon pen or pencil. In all such cases, however, the element of friction is so apparent as to make their use prohibitive. The patent granted to W. C. Patterson, No. 333,777, issued January 5th, 1886, and another to myself, No. 406,678, dated July 9th, 1889, are the only ones which cover the use of paper which is sensitive to light in combination with electrical measuring devices. I regret that my time will not allow me to describe several other meters which have been presented to the American public, as well as meters of recent foreign production. The subject matter is too extensive to include in one paper, and probably may be taken up at some future day and more fully presented.

Discussion.[1]

Prof. Thomson:—I am a little at a loss to know just exactly what we have under discussion, whether it is the meter which is before us, or the various meters that are mentioned in the paper. If I look upon the matter in the broadest way I might perhaps be able to say something as to each type of meter mentioned. But that would be rather a lengthy procedure and as one form of meter which I have designed is mentioned, perhaps I had better confine my remarks principally to that.

In the first place, Prof. Forbes made a very apt remark in connection with this meter, and that is, that it ought not to be called a watt-meter, which is perfectly true. Watt simply indicates a rate. But it is not called a watt-meter. It is called a recording watt-meter; that saves it, that is, it records the rate and integrates the rate for the time. In that sense, of course, it would be proper to call it an integrating watt-meter, recording the rate and multiplying it by the time.

The paper before us speaks of this form of the meter, which, by the way, has of course been designed for certain purposes, to fit certain conditions. One of those conditions was to give the customer an opportunity to read for himself (just as he does with the gas meter) a record and to understand it. Now, we found this in a good many cases an important matter, and our efforts were spent largely to secure that end, to get as it were, an instrument that would represent the gas meter recording on a dial the use of current energy.

The instrument, of course, has its limits; that we must fully recognize, but that it does good service inside those limits perhaps is also perfectly true. It will, of course, not readily respond to one volt or to 10 volts, perhaps, but it will respond to conditions of considerable variation in the voltage as well as to enormous variation in the current. Such meters have been constructed which will range from one to 200 amperes. This, of course, is quite a wide range, the error being only a few per cent. on the lighter loads. The accuracy for the heavier loads is almost exact, as exact as such mechanism could be expected to be. The accuracy of the smaller load meters is all that could be desired, those taking from 10 to 30 amperes for the heavy loads are practically accurate, and the sensitiveness is, of course to a fraction of one ampere. It sometimes goes lower than a quarter of an ampere.

Now, this matter of the range which a meter can work under is, of course, an important one; and I know of no type of meter which has given that much range with the same accuracy, at least so far as my information goes. It is true that the principle of the meter is not new. I claim nothing especially for the principle. I simply claim the working of that principle into a

1. By Messrs. E. Thomson, Birdsall, Van Trump, Weston, Rice and Mailloux.

device which is useful in the industrial applications. There are features about the meter which did not appear in any former arrangements and these features I think are features which have made it a success. The same criticism might be made on the meter before us—that the principle of it is not new. The new feature perhaps is the recording by a photographic method as distinguished from recording by other methods I remember distinctly. I think it was in 1886 or 1887, making a meter which was substantially the same in principle, only the record was made on a sheet of paper by punching little pin holes at intervals. There was no friction during the setting of the ammeter portion of the apparatus. The friction was as little as it is in the meter before us and the little point that passed over the paper was clear of the paper until an electro-magnet struck it a blow and made a little indentation, the point jumping up again and being ready to take a different position. That eliminated the element of friction, and I had in that m ter an arrangement for doing away with clockwork which worked very satisfactorily in the meter constructed It is nothing more nor less than a pendulum which is self starting. There was an electromagnet which pulled upon an armature and a pendulum self starting. This depending lever or the pendulum worked a pawl [illustrating] which gave us the motion or winding of the paper. The pendulum at rest was always stopped in a position ready to start and it broke its own magnet circuit, or worked a set of contacts for the magnet which kept the pendulum in motion so that the rate of going was perfectly regular The little recording point was mounted on a spring connected with the ammeter which latter was merely a little piece of iron on a vertical shaft inside a coil, and the coil and the iron shaft so arranged as to get substantial uniformity of movement throughout the range of current. The little recording point was over the strip of paper and the electromagnetic hammer was above it, and at every revolution of this ratchet wheel worked by the pendulum which constituted practically the whole of the clockwork, the magnet armature was drawn and struck a blow which brought the point down to the paper. The paper as it came off would have a series of dots the position of which would change according to the load, and the same process of integrating this curve would be used as in the meter before us. For the purpose of recording, we did not waste any energy as is here done in working an incandescent lamp for photographic action in the meter. No doubt the lamp used could be diminished in size. But if it should give out from any cause, it is not replaced at once or until some one discovers the difficulty. Until then the record fails. I do not care to go into any further criticism in relation to this matter; but I think when we look at these things we should be prepared to look at them from all sides.

Mr. Walker:—The remarks of Prof Thomson are undoubtedly very explicit, and very true. We do not claim for this

meter that it is a new principle, beyond the manner of securing the record of the action of the ammeter needle. I am aware that devices such as Prof. Thomson has explained in which a slip of paper has been marked by the aid of the ammeter needle. have been tried very frequently as well as the use of a siphon pen on the needle point, I remember in looking over this question to have discovered one which was proposed by Mr. Edison. Mr. Edison provided a siphon pen which was carried on the ammeter needle and a strip of paper which was kept moving by a clock movement or suitable device and a magnet which at intervals of half a minute or half a second would bring the paper against this siphon pen and get a dot. However, in case of an interruption of an hour or two it is very likely that the siphon would clog up and there would be no further dots. The meter described by Prof. Thomson, would have the same difficulty inasmuch as the pointer might pierce through the paper and fail to act thereafter. The major criticism that has been made on the Walker meter is the possibility of the breaking of the lamp in the meter. In the exhibit you see a lamp burning very brightly, the candle power used there however is not at all necessary. In fact, a lamp burning at one candle power or a sixteen candle power lamp of very high voltage will be ample. Say you are supplying a meter on a 110 volt circuit; place in the meter a 150, 160 or 175 volt lamp and you have ample light to photograph the action of the needle, and it is then quite likely that the lamp will not break. In other words lamps burning under their voltage are very apt to have considerable life. If, however, this question should prove to be a serious objection, it is very readily overcome by putting in a double filament lamp or a contrivance for operating two small lamps. The power required to operate the clock mechanism depends entirely upon the size of wire used in the winding magnet, and as said before the candle power of the lamp is of very slight consequence, inasmuch as the burning of a match would be ample to give us all the record necessary.

Mr. Van Trump:—The distinguishing feature of this instrument is that it is a recording ampere-meter, and I should like to ask Mr. Walker what he has to offer us in the way of a recording voltmeter. The use of such an instrument would immediately suggest itself to those who have to do with central station management. Such an instrument placed on lines in a station and a duplicate instrument used at different points on the mains would give us cards which would give us very valuable data in the distribution of our copper on the mains. And besides that a recording ampere-meter to automatically register the load on a central station is a very nice feature. It does away with the unreliability of having an attendant mark up on a load chart the load on the station at different times; so that we can at any time tell exactly what we have done and what our proportion of fuel to output has been.

MR. WALKER:—I would state that we have in contemplation a system based on the principle referred to by the gentleman who has just spoken. For example, we propose to devise instruments sufficiently large to measure the amount of current produced at the central station and smaller sizes for distribution service and any difference between the products of the station instruments and those on the line would naturally be the loss on the line in ampere current. We are also at present at work and have very nearly ready for placing before the public a recording voltmeter operating on precisely the same plan. Such an instrument was suggested to us by the usefulness which the recording ammeter has shown By having the recording voltmeter at your station and at various points on the line it is absolutely certain of securing the exact drop of potential on the line, and will besides be practically a detective on your engineer and electrician at your stations, showing whether they are keeping up the proper electromotive force all the time. In other words, the superintendent will at the end of the month have a perfect check against his engineer and electrician.

MR. BIRDSALL:—The subject of recording meters at stations has, I think, come to every station manager, and they have all felt the need of them, and in the course of my trips among the various stations of the country I saw the great need; as in a great many of the stations they made a curve of the load every day by hand. Recognizing such a want, about four years ago I started out to design a recording volt and ampere-meter for such uses The great object with all these instruments is to avoid friction. The use of a pen or pencil is not feasible; and after talking with a good many station managers I gave up the idea that is used in this meter—that of photographing the record. The station managers or the superintendent would not bother to develop the slip. They want something that is in ink or pencil like the steam engine indicators, something that they do not have to bother with afterwards. And after working on this matter sometime I hit upon the plan of running the paper over the face of a magnet. The pencil or pricking point has a small mass of iron on it. By clockwork the circuit of this magnet under the paper was closed at every five seconds, I think, and the pencil was in that way brought down on the paper. The magnet was of a sufficient size, so that the field was practically uniform within the range of the movements of the small armature. I attempted to get a patent on it, but I found that Mr. Cuttriss had patented almost the identical thing for a siphon recorder. That of course, produces a series of dots. By making the intervals of striking sufficiently close together you practically get a line. So far as I know there is nothing of a practical nature of this sort in the market at present, although the need is felt by station managers. The question of friction is the great item, especially on the voltmeters. Of course, the paper in the meter need not be wider than a ticker tape on a zero instrument.

MR. VAN TRUMP :—I would say in connection with the matter of friction that some years ago in looking around for just such an instrument—a recording voltmeter—and not being able to find such an instrument for our station in Wilmington, we thought that the best plan was to have one made, and we have done so on the Cardew plan having three wires. In that way you have plenty of power to drive your pencil and we have three wires, one for each side on the Edison system and one for the zero. Of course, such an instrument does not give absolute values, but it points out fluctuations in electrical pressure and may suggest a remedy.

MR. EDWARD WESTON :—I was particularly struck by one remark in regard to the Thomson watt-meter, which I think gives rise to some misunderstanding of the operation of the device. It seems to me impossible for a Thomson watt-meter to run unless current is passing through the main coil spring. It seems to be utterly impossible that any action of the retarding mechanism would take place on the armature to produce motion of the armature by simply the shunt coil and if such a thing did arise I should attribute it to something totally different and not to the permanent magnets in that instrument. It would indicate that insulation of the spring in which that armature was placed should be looked after. If the current was flowing through the meter which should not flow there, there was some very bad work somewhere in that installation The form of meter having the dial seems to me to have a great many very marked advantages over any other type of meter known. In the first place, the public are more or less acquainted with the gas meter A great many of them understand reading it. It is there; a fixed thing from month to month. You can make note of the changes on the dial and have a check upon the gas company, assuming, of course, that the meter is moderately correct. On the other hand a system of metering which does not enable the consumer to check the results, to check the consumption, seems to me to be open at least to some objection, and possibly considerable objection and suspicion on the part of the consumer. Hence you can readily understand why so much time has been spent in endeavoring to bring that type of meter closely approaching the gas meter, to the state of perfection to which it has been brought to-day. The type of meter before us to-day has certain excellent features and is certainly a very ingenious piece of mechanism and I have no doubt will record quite accurately. It is free from a great many difficulties and defects where recording is accomplished by mechanical means. The recording mechanism does not induce any friction But it has, of course, the disadvantage that the record has to be taken back to the station house and developed. It is true that that record can be produced at any time for inspection of the consumer in case of dispute; so it might be said to be proof positive of the correctness of the bill rendered.

But it seems to me that it would never be accepted with the same degree of confidence that it would if the consumer could make these calculations and did make them himself and thus kept a real check on the meter. It has to go out of his hands to be developed and unless he has a person to see that it is developed he might readily charge fraud on the part of the supplying company. The public are somewhat tender on this question of meters anyhow. They unjustly, I think, blame meters for things that do not exist, or only exist in their own imagination. In the gas business that is exactly the same, The gas meter is not an instrument of absolute precision, but it is a good practical instrument. Any one who has investigated it will admit that it is a very reliable thing indeed, if kept in repair. It does not need very much time to keep it in repair, or very much attention. And if electric meters could be brought to about the same state of efficiency that gas meters are—and I think they have already reached that stage, and even excelled that—it is sufficient for the requirements of the art. I have entirely forgotten the patent to which Mr. Walker referred a little while ago, as bearing upon the same form of meter that Prof. Thomson has invented and therefore I cannot speak with any confidence about the nature of the device I used there. But I have had occasion to examine the Thomson meter, and I will say this, that I never got such results as Thomson did, and I am inclined to give him the most credit for working out the details of the appliances and getting it to the state of perfection that he has brought it to. It means a great deal of close study to bring appliances of that kind to a condition where they can be used practically and trusted perfectly, and while someone else may have got an idea too, let us give the man who has brought the thing to a state of perfection, credit for that. The objection to the clockwork meter is, of course, its first cost. The most accurate meter I have ever seen in my life was the Aron meter, but it requires a little more skill than we generally meet with to set it up, and it requires considerable skill to adjust it so as to get it to yield good results. It will yield admirable results if handled right—better than any I have ever seen.

There has been some discussion about voltmeters, and that introduces a subject which is somewhat interesting to the members here. It is generally supposed that in recording voltmeters friction would be the principal source of error. Now that is not so. I think that you can get the recording measurement so perfect and so simple in character that you may say that you can practically eliminate friction from that part of the apparatus. The temperature changes are very frequently overlooked and they usually amount to a very large sum indeed. They can be compensated for to a certain extent, and possibly as we gain more experience, perfectly so. But there is a good deal of misconception about that question of compensation of instruments.

A short time ago I devised a little apparatus for compensating for change in temperature, which consisted substantially in a little thermometer, the bulb of which extended over on to the coil of the instrument so as to indicate the temperature of the coil. The thermometer bulb is supposed to be small and the instrument as actually made was not as small as it should be, and it responded very readily to slight changes in the temperature of the coil. You could read the thermometer from the outside of the instrument and to avoid any computations I put on a little device by which you could turn an index around to a point corresponding with the temperature indicated on the thermometer; that is to say if the thermometer showed 75 degrees. then you would turn the indicator around to a point indicating 75, and you compensated without any calculation whatever. I was somewhat surprised, however, by the publication of the thing—it got into the journals before I was ready to have it appear. An article was printed in one of the English journals, I think it was the London *Electrician*, saying that such a thing as that was of no use; it was easy to compensate an instrument without any such thing as that. Now the writer of that article had not given as much thought to it as he ought to have done. If he had he would not have expressed such things as he did in the article. I did not take any notice of it. Well, it overlooked one important thing—that you might compensate for air changes quite readily and yet not compensate for changes produced by the heating effect. Now to get an instrument properly compensated you must compensate for both things—changes in the instrument itself due to the changes in the atmosphere, and changes in the temperature due to the actual passage of the current. When you have got a device that will accomplish that, then, of course, you do not need any mechanical device to switch into a certain fixed point. It just happened that at that time I did not have a device of a character which I could put in a satisfactory form. I think now I can do it. It frequently happens that criticisms and remarks are made about the operation of an apparatus which are wholly unjustified and untenable upon a close study and examination of the things themselves. I think this same is true of the Thomson watt-meter. I have had occasion to listen to a good deal of talk about the Thomson watt-meter. Some of it has been of pretty severe character too, and the instrument has been severely criticised. Now I do not think the instrument was so much to blame as the parties who were using it. I am frank to say the examination indicates that no such errors as they speak of come into that instrument unless they abuse it. In the Thomson instrument I do not think the proportions are the best imaginable, to get the very best results. But I think it is only a question of time when they will make quite important changes in detail which will make it far better than it is to-day. One criticism I should make of it is, that the moving parts are some-

what heavy and they should be reduced in weight as much as possible. That can be done by the substitution of aluminium for copper in the retarding device, that is on the disc. The form of retarding device might be changed. Instead of adopting a disc, a cylinder might be used running between the poles of a powerful magnet and you might get exactly the same retarding effect by bringing the poles very close together, having a central core, and there reduce the weight very much. I should be very much afraid of errors creeping in from the mass of matter to be acted on there by the current, especially with low loads. I should not think much of it with heavy loads; I think there you would have power enough to overcome it. But with small loads and but three or four lamps, I should be afraid that the friction on the pivot would not be a constant element but would be quite variable in use. I do not think that is anything against the instrument, because I think it can be entirely overcome.

The Thomson instrument, of course, has the advantage that it can be used for either alternating or direct currents. Now, so far as I remember it, no instrument that I had anything to do with in my earlier work on meters was capable of those applications They could only be used for a direct purpose. I do not remember doing anything in that line where the meter produced would be available for both purposes.

Prof. Thomson:—It seems perhaps that I should make a few remarks. I think Mr. Weston has shown by his remarks that he has spent a good deal of thought on this very problem and understands the conditions most fully. It is not an easy thing, and I can say it after considerable experience, to get commercial results such as we may get in laboratory testing. You may make a laboratory apparatus and produce it as perfectly as possible and put it out into commercial use and you will find difficulties. I have frequently had that experience, and that experience has come to me more forcibly perhaps on the meter question than on any other. It is not until you have had commercial experience that you can make those little refinements which are then shown to be absolutely necessary.

Mr. Weston has said very truly that the weight of the parts in the meter as constructed by us has been rather excessive. We are using every effort to reduce it. We are using much lighter discs and getting as strong as possible a field for it to move in, that is, the retarding disc.

There are a good many points which cannot be brought up in this discussion; but I do not know that they are important to discuss at this present time. Experience, of course, will tell the story in the end. There is just one point I wish to make before closing my remarks, and it is, that it is not the case of getting a meter that will meter, but it is the case of making such a thing that you can put in for the price. It is the case of making a thing that

we can put on the market which will not cost too much. You may have a very fine mechanism; you may have an excellent device for the purpose, but if it costs $50 nobody will take it. You must be able to make it for $7 or $8. I hope to be able some day to make a good meter for three or four dollars. [Applause.] I do not know that I shall succeed—do not applaud yet. But when we have done that, I shall consider that the meter question is probably laid aside. But it is one of those questions that has been with me for a long time, and I have had no problem that has demanded more time, care and attention than that very problem. I would much prefer to design a thousand horse power dynamo and see it through than to see a meter through any day, where you are obliged to have a sensitiveness to extremely small currents and also a sensitiveness to large currents. The effects of those currents tending to increase as the squares or in some such proportion, our elements of friction of course become excessive when we get down to the small currents and are practically eliminated with the larger currents. Our object then was to get proportionality between the various currents in their effects and secure for the meter as great sensitiveness as possible for the lighter loads. Having secured that, the rest is not so difficult. Of course, we are still working at the problem. We have gone through thousands of experiments and thousands of tests on this very question and we never know just exactly when we are out of the woods.

Mr. Mailloux:—I want to add a little bit of experience in relation to meters, which goes to show that while a dial meter may be very easy to read, it is not always satisfactory to the producer. Rather an anomalous case came under my observation. I put in a plant up town, in an apartment house, where the tenants wanted to pay for their light by meter. They did not care to pay for it in their rent. It was a large apartment house, consisting of twenty odd suites of apartments. Some of the tenants maintained that as they were in Europe or elsewhere half the time, they did not care to pay for electricity that they were not using. Consequently, the only recourse was to put in meters. These were put into the air shaft usually, or the nearest place to the various suites of apartments so as to have them as much out of sight as possible. They were clockwork meters, differential clockwork meters of the type which has been described here and referred to in connection with this paper. There is much building going on in that up-town district, and after a time they began to excavate and blast in the immediate neighborhood. When the engineer called around to take the meter he found that he was owing the customers for electricity instead of their owing him. In other words, the meter was going backwards and he found that not only had the previous record been obliterated, but that he was actually owing more than he had ever had. Upon an investigation of the strange

phenomena we found that one of the two clocks had stopped and it happened to be the wrong clock. If it had been the other the customer would have owed the company several times more than he had used. As it was, the company was literally owing the tenant for electricity. The experience was repeated several times and it was found that we had practically to abandon the meter, because it was so unreliable. In some other instances it did actually happen that the other clock stopped and it would charge more in a month than could be consumed in a year. So we had to go by average and guess work, and it was almost as easy to guess without the meter as with it. I have recently put in a Thomson meter, which, as far as I can see, gives perfect satisfaction. Of course, I can see that since the meter is really a motor which has got a sort of Prony brake to it, the coefficient of friction is likely to vary a little and that it may not be so accurate on light loads; but I consider that when it approximates anywhere near 60 or 75 per cent. of its capacity it would be practically correct under all circumstances. I do not see why it should not be correct.

PROF. THOMSON:—There is just one remark I intended to make when I was up before, and that is in explanation of the statement that the meter has been known to turn without load. That is true. The meter was originally developed for alternating currents and was used for alternating currents for a long while, the demand being in that direction; and when it was put upon continuous currents it did turn and keep turning sometimes in one direction, sometimes in the other direction, when there was no load. The side frames were made of cast-iron and while under load with continuous currents, they would get permanently magnetized with two poles opposite the armature. Of course the field is exceedingly weak, must have been exceedingly weak, but it was sufficient to cause rotation of the armature. It shows how little force was needed to rotate the armature, the armature being 2½ inches in diameter and the frames about 8 inches apart. That however, is eliminated by making the frames of brass instead of cast-iron.

PROF. ANTHONY:—I would like to inquire of Prof. Thomson if the meter would not run in the earth's magnetic field.

PROF. THOMSON:—We have got certain indications of that. We have thought sometimes that it had a tendency to turn, but the coefficient of friction is fortunately generally large enough to prevent that.

PROF. ANTHONY:—I had a little experience with one of the meters, and I never tested it for that, but I know that the two main coils were connected in the armature circuit so that there was a little tendeney to run any way. Of course the idea was to overcome the friction and I set it up in such a position that the earth's horizontal lines of force were in the same direction as the lines of force developed by the main coils and the meter continued to run

when there was no current in the main wires, except the small current that went through the armature. I had always in mind to take the meter and turn it around the other way, so that the earth's magnetic field would go in the opposite direction and see what effect it had,

PROF. THOMSON:—If the meter was set east and west it would obviate the difficulty. By the way, it is true that with the very largest capacity meters, the shunt is beyond the main coils so that the shunt current through the armature goes through the main coil, and the purpose of that is, of course, to make the co-efficient of friction disappear, as far as possible. This in the instances in which the meter works at very high loads, and also (is to) be sensitive to the lowest load.

MR. WALKER:—I would like to say, in reply to Mr. Weston's remarks, that although I fully realize the desirability of having a satisfactory dial meter I fail to see how the customer is to be convinced of the correctness of its operation. I do not see, for example, how the customer will very readily understand what 25,000 watt hours mean, or what we would understand by a certain number of milligrammes of deposit on the electrode. On the contrary, he can quite readily understand a diagram chart such as is made by the meter before us. If you will place the Walker meter on a 110 volt circuit each 16 candle power lamp will take in the neighborhood of half an ampere; consequently, if the record should show that 10 amperes, for example, were consumed, say for five or ten hours, why it could be very readily explained to the consumer that he had been burning 20 lamps for five or ten hours. As I stated in my paper the manner in which the result is attained is so readily explained to the customer that I do not see why he should not understand it.

Now, as to my remarks on the Thomson watt-meter, I merely wish to add that I have endeavored to state facts concerning this meter, based upon personal experience in operating the same. I will be frank to say that I have one of their early meters in use, and probably the results obtained therefrom are not as satisfactory as from those now manufactured. In looking at some meters in operation at the Providence convention, which were of the most approved pattern having brass frame instead of iron, I found the tendency to operate after the load had been turned off, was still prevalent.

Mr. BIRDSALL:—Mr. Walker says that he thinks the customer would not care much what the record was, as long as it was explained to him, or that he would not know what watts meant, etc. That is all very true. But I have had experience with the "dear public," so to speak, in connection with a chemical meter and the great cry always was when there was a dispute about the bill, that the thing had no dial on it—no figures—that those little bottles did not show them anything and when you branched out into the number of milligrammes of zinc deposit per ampere,

why you generally talked alone. I do not think it matters what the dial registers, so long as you have a dial. [Laughter.] The customer does not care whether it is lamp hours or watts or horse power. The mere fact that he has a number—that he has 75 of something, is all that is necessary. He knows he pays so many dollars for so many things, and he is willing to pay the money as long as he is pretty positive that he gets them. The accuracy of the meter I do not think is such an awful thing to strive after, because if the dial says 75 and the man burns 60 he will pay for the 75.

Mr. E. W. Rice, Jr:—Before this discussion is closed I think it is proper to remark that even in cases where the Thomson meter has been found to register current passing through the meter the rate of such registry is exceedingly small. I do not think it exceeds (1-18) one-eighteenth of one lamp per hour so it would take a long time to run up a large bill against the customer.

In regard to Mr. Walker's remarks in reference to the dial, the last speaker has stated what I had in mind so fully that I do not know that I could add anything to it. I have one of the Thomson meters in my house and it is quite a convenience to be able to read the dial and find out how many watt hours I have burned in one week, and caution people in the house not to leave the lamps burning in the garret and cellar.

In reference to the fact that a customer would not know what a watt hour is, I think it is also equally true that the average customer has no idea what a cubic foot of gas means, that is, I do not think that the average customer knows that a gas burner consumes say five or six cubic feet of gas per hour. He knows that he pays one dollar and a half or two dollars per thousand cubic feet. Now that is exactly the case with the Thomson wattmeter. The register reads watt hours. The price is so much a thousand watt hours. Now it curiously happens, as first noticed I believe by the speaker, that one thousand watt hours in a lamp of the usual efficiency will give about one-tenth the amount of light given by one thousand cubic feet of gas, so that the customer, to be on an equal basis, can be charged one-tenth the amount for one thousand watt hours as is charged for gas per thousand cubic feet. That is, a thousand watt hours is roughly equivalent to one hundred cubic feet of gas. So we say twenty cents a thousand for electricity is the same as gas at $2.00 a thousand.

The customer likes it because it seems more natural to pay by the thousand, and electricity at eighteen or twenty cents, appears cheaper even if it is the same, as gas is one dollar eighty, or two dollars.

Mr. Weston:—I would like to explain a remark I made some time ago. I questioned the fact of the Thomson meter running under low current through the main coil. I had had no experience with the iron frame meter. I did not know they had an iron frame. If I had known it I should not have made the re-

mark. As to the question of the meter revolving in the earth's field, I think it would be a nice thing to get the meter to run in the earth's field.

Prof. Thomson made some remarks about the cost. Now, I think he is wrong. I should take issue with him on that. I think that the best thing electrical engineers can do in this state of the business is to produce the very best thing that they know how, and trust to the future for returns. It is true that one company starts in and puts a meter on the market at say $20 or $18. That meter may be good or it may not be very good; but I think that it is not very good; that the man who follows along and tries to get up a meter should make a very much better meter and get a price appropriate to the superiority of the instrument. I think that question of low cost plants and apparatus in electric lighting means in the end very high cost. Many people have paid a good deal of money for that kind of experience. Cheap engines, cheap boilers, cheap wiring—everything cheap, means in the end everything very dear. Now, you see what the effect of it is. All through this country stations are being remodelled to get in the very best appliances—triple-expansion engines—the maximum economy in every direction consistent with good work; and the meter I think should be given just as much consideration as the rest of the plant. Do not make the cost so that you can not make the meter really fine and good. I would of course increase the cost of the meter and put better work in it, and make the best I could.

Prof. Thomson:—I would say that I agree most fully with Mr. Weston in regard to this matter. Those are the sentiments under which I have worked, but when I make that argument to the Boston office of our company, the answer is, yes, and so and so sells a meter for so and so, and by the way his has a dial on it. [Laughter.] We must do something about as well, even if we have to sacrifice something in the perfection of the meter. I hold just as Mr. Weston does that you cannot do the thing too well. It is a nice problem anyhow. It is a delicate affair at the best and you cannot get the meter too good. You must look to cheapening afterward. But that is an argument which the business managers fail to see, and particularly I am afraid that those in charge of stations who have to buy meters perhaps fail to see it still more. That is they have not that high sense of justice in regard to the meter. They want something that they can put out to satisfy the customer that it has got a dial, and then bring in a bill, and if it costs seven or eight dollars and they put that kind in and it can be read every now and then and a bill calculated it is all right so far as they are concerned. Just as in the case of engines and other appliances which are now being remodelled, if they do have these imperfect things the day will came when they will have to buy them over again and pay a good price.

There is one other point. I would say here that, of course, in an electric meter we measure electricity, or at least we measure the volts and the amperes. In the gas meter it is different. We measure whatever comes through, whether it is air or gas. But we cannot adulterate our electricity. [Applause.]

THE CHAIRMAN:—As the time is limited, and the programme still quite extensive, we had better pass to the next paper which was left over from this morning's session. It is upon a subject of great importance, but which has never been touched upon before to-day, and a matter which is interesting in being unique and deculiar, and that is "A Study of the Fluctuations of Electromotive Force in the Open Coil Armature of a Constant Current Machine," by Mr. M. E. Thompson of Cornell University.

Mr. Thompson then read the following:

A paper read at the General Meeting of the American Institute of Electrical Engineers, New York, May 21st, 1891. Vice-President Crocker in the Chair.

A STUDY OF AN OPEN COIL ARC DYNAMO.

BY MILTON E. THOMPSON.

Of all the dynamo machines in use at the present day, perhaps the internal action of none is so little understood as that of the arc lighting machines of the open coil armature class. Much concerning the regulation and general behavior of these machines seems utterly at variance with what one would naturally expect from a superficial examination of the design and construction, and it was with the idea of throwing some light on this seeming mystery that the investigation to be described was undertaken. In carrying out this work no attempt has been made to determine the efficiency of the dynamo, or to determine an answer to the question—Is the machine well suited for arc lighting? The vast numbers of these dynamos in daily use in all parts of the world is a practical answer which must carry more weight than any which might be suggested by a theoretical study of the machine. The experimental work of this investigation was carried out under the direction of Professor Nichols in the laboratories of Cornell University, during the past winter, by the writer, with the assistance of Mr. H. J. Hotchkiss and Mr. E. L. Morley, and due credit must be given to these gentlemen for any points of interest which may have been brought out by the investigation.

The object in view was to determine accurately. by taking instantaneous values throughout the revolution, the character of the external and internal currents and electromotive forces.

The capacity of the dynamo in question was about ten arc lamps of 1,200 c. p.

In order to carry out this work it was necessary to build a special commutating device for taking instantaneous values of

current and potential. Professor Ryan was the first to introduce this method at Cornell, which is a modification of that described by Joubert in 1881,[1] and since used by various observers in alternate current work. The device he used was described at length in a paper read before the Institute[2] on the 17th of December, 1889. The device consisted essentially of a metal tongue mounted upon the shaft of the dynamo and revolving with it, and making contact once each revolution with a small metal spring mounted upon an arm whichwas so arranged that it could be revolved about an axis coincident with that of the armature of the dynamo and secured in any desired position. By properly adjusting the position of the arm carrying this spring, contact could be made to take place between the spring and the tongue at any desired point of the entire revolution of the armature.

Three sets of curves of exterior current and potential were taken with three different loads upon the machine, and three sets of curves of current and potential through a single coil were taken with the same loads. In order to keep the electromotive force as nearly constant as possible, and prevent variations while a set of observations were being taken, the machine was loaded with a german-silver resistance. The loads consisted of about 20, 50, and 80 ohms respectively, and were equivalent to about three, six and ten arc-lamps. The current for all loads was supposed to be practically constant, and an ammeter placed in the circuit showed but little variation from the normal current of 6.8 amperes. A condenser and ballistic galvanometer were used for making the measurements, and Fig. 1 shows a diagram of the connections of the dynamo and also of the measuring apparatus. Referring to the diagram, A is the armature, FF the field coils, M the regulator magnet, C the controller magnet, and *r* the carbon resistance used to prevent excessive sparking at the contacts *k*; P is the positive terminal of the machine and N the negative, R is the german silver resistance which constituted the load for the dynamo, and *i* is a resistance of about one ohm placed in the circuit to serve as a basis for the current measurements. LL and SS are the brushes of the dynamo, which bear against the three segments of the commutator, as shown in the diagram; D is a condenser, K a discharging key, and J a small spring switch; G is a ballistic galvanometer, through which the condenser was discharged, and T

1 Joubert; Etudes sur les Machines Magneto-Electriques: *Annales de l'Ecole Normale-Superieur*, 10, 46 pp., 1881.

2 TRANSACTIONS, vol. vii, p. 3.

is a shunt box for regulating the throw of the galvanometer and bringing it within proper limits. From the negative terminal of the machine, N, runs a wire to the commutating device, *c s*, and from that to the switch, J. From the positive terminal, P, and the binding post, B. run wires to *f* and *g* of the switch, *fgh*, as shown. From *g* of this switch runs a wire to the discharging key, K.

Before describing the methods of taking the measurements, it

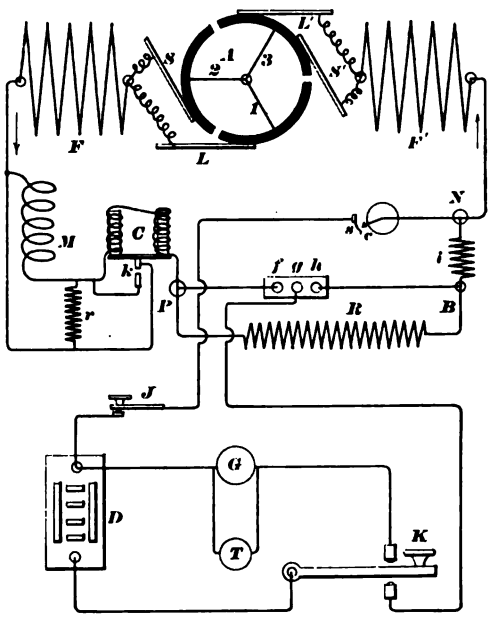


Fig. 1.

may be well to review briefly the operation of the dynamo and its regulation so far as is apparent from an outside view. The current, leaving the armature through the brushes, L and S, passes through the left field coil and thence through the regulator magnet and controller magnets to the positive terminal, P, of the machine. Passing around through the external circuit. in this case the resistance, R, the current returns to the machine by the negative terminal, N, passes through the right field coil, F, and through the brushes, L and S, to the armature, thus completing the circuit. The regulation of the machine is effected as follows: The controller magnet C is so adjusted, by means of a spring, that a

current of 6.8 amperes will just raise the cords of the magnets and separate the contacts, *k*. This permits the current to flow through the regulator magnet, M, and raises the regulator arm, thereby shifting the leading brushes, L and L′, forward, and the following brushes, S and S′, backward, and thus reducing the potential furnished by the machine. The current is, of course, thereby diminished, and the cores of the controller magnet drop, closing the contacts, *k*, and thus short-circuiting and cutting out the regulator magnet. The regulator arm then falls, under the action of gravity, and the leading brushes being moved backward and the following brushes forward, the potential is raised until the current increases sufficiently to once more open the contacts, *k*, and again cut in the magnet of the regulator. The controller magnet acts simply as a relay, and is continually cutting the regulator in and out of the circuit, and in this way the current is maintained constant, regardless of the varying conditions of the external circuit.

During these experiments the regulator was allowed to act just as in ordinary practice, but each load was constant and practically non-inductive. Three sets of curves of external current and potential were taken and the measurements were made in the following manner: The commutating device, *e x*, being secured in a fixed position, *f* and *g* were connected. The discharging key, K, being then depressed, switch J was then closed and in a few seconds the condenser became charged with the difference of potential existing between the terminals of the dynamo at the particular point of the revolution of the armature represented by the position of the commutating device. Switch J being then opened, the discharging key, K, was allowed to rise, and the condenser was thus discharged through the galvanometer, G, causing a deflection which was duly noted. The connections of the switch. *fgh*. were then changed so that *g* and *h* were joined, and in this way the condenser was charged with the difference of potential existing between the points B and N of the external circuit, at the same point of the revolution as the first reading. The condenser was then discharged through the galvanometer and the deflection noted as before.

After moving the contact spring, *s*, forward ten degrees, the measurements just described were repeated, and so on until the contact had been moved entirely around the circle.

The condenser and galvanometer being properly calibrated, it

was an easy matter to calculate from the data obtained as above, the difference of potential between the terminals P and N, and between B and N, at 36 points of the revolution of the armature. The resistance between B and N being known, the current was readily determined from the fall of potential. The results of these measurements are given in Table I and are shown graphically in Fig 3 and will be referred to later.

After taking curves of the exterior potential and current, the apparatus was arranged for measuring the potential and current in a single coil of the armature. Coil number 2 was selected

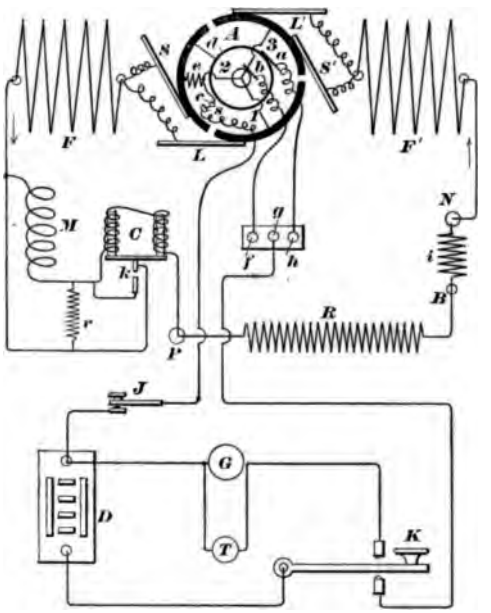
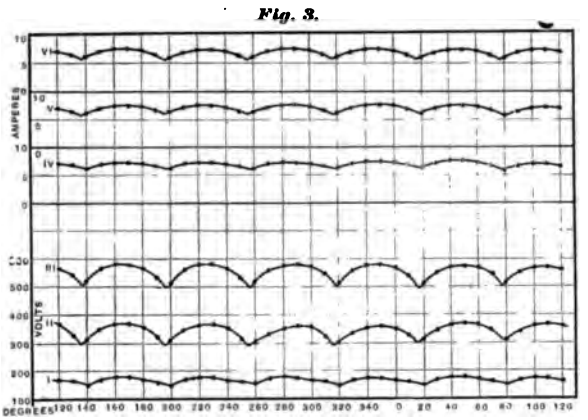


Fig. 2.

as likely to present results nearest the average of all three of the coils, and the apparatus was arranged as shown in Fig. 2. The circuit of the dynamo and its loads were exactly as for the first measurements, and the connections of the measuring instruments were as shown in the diagram. Referring to Fig. 2, *a* is a small brush which bears against a ring *d* secured to the commutator but carefully insulated from it. This ring is connected to the commutator bar of coil number 2, and thus gives metallic connection with this bar at all points of the revolution. A small brush *b* bears against a similar ring wound round the

armature shaft but insulated from it. This last ring is connected to the three inner ends of the three coils, and thus a continuous contact is made with these ends. The outer end of coil number 2 was disconnected from the commutator bar and a small non-inductive resistance *e* was inserted between the end of the coil and the bar. The junction of the coil and the resistance *e* was connected to the tongue *c* which revolved with the armature shaft. Once during each revolution the tongue *c* made contact with the small spring *s*, from which a wire ran to the switch J and thence to one terminal of the condenser D. Wires ran from brushes *a* and *b* to the switch *f g h*, and from this switch a wire ran to the discharging key K, as shown on the diagram. When *g* and *h* were connected and keys J and K depressed, the condenser was charged with the difference of

Fig. 3.



potential existing between the ends of coil 2 at the particular point of the revolution represented by the position of the contact spring *s*. The condenser being then discharged through the galvanometer, a measure of this difference of potential was thus obtained. Switch *f g h* was then changed so that *g* and *h* were connected, and measurements then taken with the condenser and galvanometer gave the difference of potential between the ends of the non-inductive resistance *e*, and from this the current in coil 2 was calculated. Readings were taken in this way at distances of five degrees apart all around the revolution and the results are given in Table II, and are shown plotted out in Figs. 4, 5 and 6, three curves at three different loads being taken as before. It is not the purpose of the writer to go into the theories of cause and effect as exemplified in these

curves, but to simply present them, calling attention briefly to some few of the more noticeable and interesting features connected with them.

Turning now to the results obtained, in Fig. 3, curve I represents the difference of potential at the terminals of the dynamo throughout the revolution under a light load, and curve IV is the corresponding current. Curve II is the potential for medium load and curve V the corresponding current, while curves III and VI represent the potential and current at full load. It will be noticed that the curves for medium and full load are practically alike while the curve for light load differs slightly in its character and is less symmetrical, and its minimum points occur slightly later than in the other curves. The curves of current and potential are practically alike, as might be expected since the self-induction of the external circuit was little or nothing. The

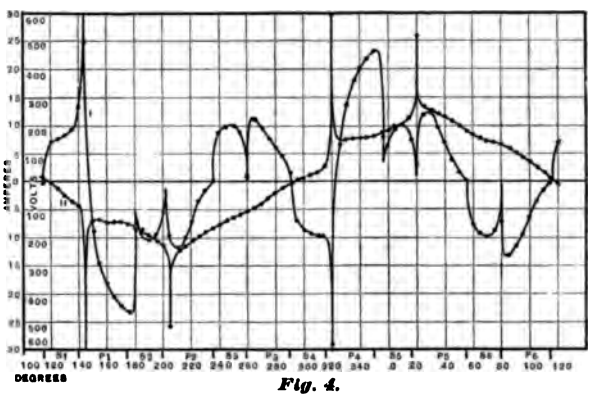


Fig. 4.

curves are all of them about such as would result from a closed coil armature with a six part commutator. It may be well to remark that in taking the observations for these curves, closer readings than are shown were taken near the minimum points, so as to be sure these points were correctly placed.

These curves show that the external current is continuous and much steadier and more uniform than is usually supposed.

Referring to Fig. 4, we find that the state of affairs in a single coil is quite different from that at the terminals of the dynamo. Curve I represents the difference of potential at the ends of the coil throughout the revolution, and curve II is the current in the same coil during the revolution. The curves shown in Fig. 4, were taken with a light load. Figs. 5 and 6 show curves taken

with medium and with full loads. For some unknown reason it was found impossible to get accurate readings for the current at full load, and so no current curve is given in Fig. 6. It is proba-

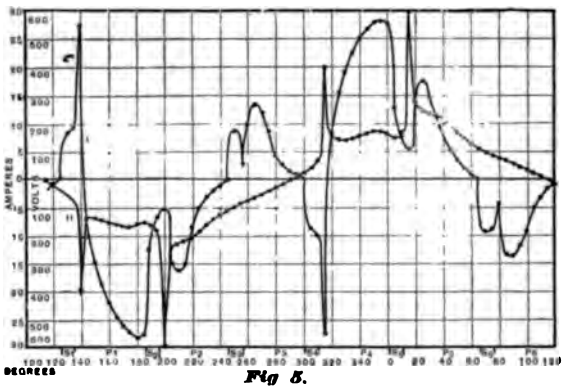
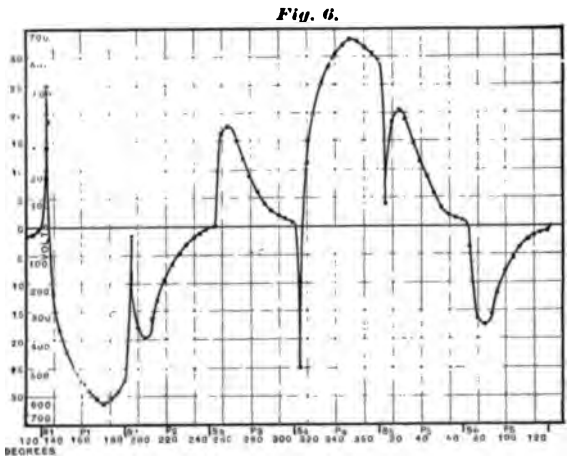


Fig 5.

ble though, from the similarity of the current curves at light load and at medium load, that the current for full load differed very little from the other two.

It will be noticed that at all loads the potential changes sign no less than six times during the revolution and falls to zero, or almost to it, as often as twelve times, while the current reverses but twice during the revolution and for the most part goes through very gradual changes. At the bottoms of the figures are

Fig. 6.



lines of spaces showing the positions of the armature during the part of the revolution represented by the curves above them. s_1, s_2, s_3, etc., are the periods of short circuit and p_1, p_2, p_3, etc., are

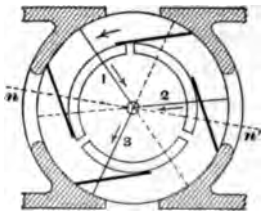


Fig. 10.

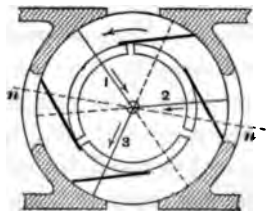


Fig. 14.

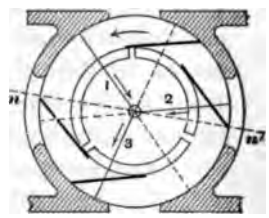


Fig. 18.

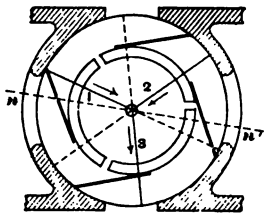


Fig. 11.

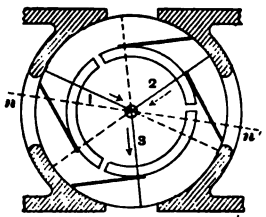


Fig. 15.

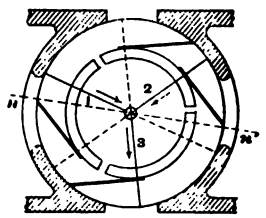


Fig. 19.

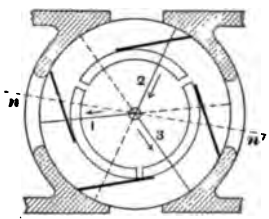


Fig. 12.

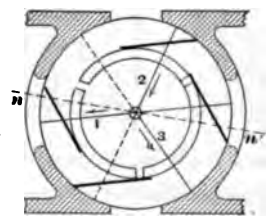


Fig. 16.

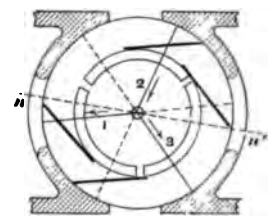


Fig. 20.

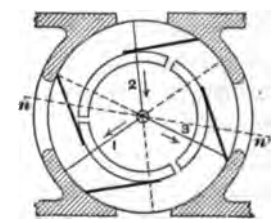


Fig. 13.

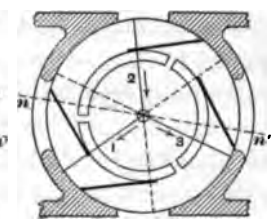


Fig. 17.

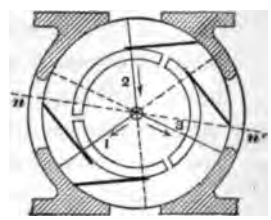
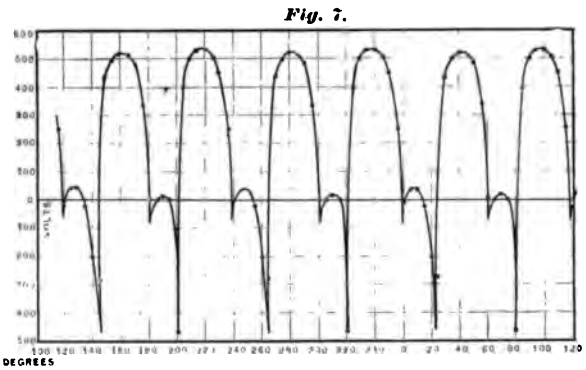


Fig. 21.

the periods during which two coils are usually supposed to be in parallel with each other and in series with the third coil. There are six periods of each kind in the course of each revolution. Figs. 10, 14 and 18 represent the relative positions of commutator and brushes at the three different loads during the parts of revolution shown at s_1 For the light load this period is about 25°; for the medium load about 10°, and for full load not more than 1°. The periods shown are not the *actual*, but the *apparent* periods of short circuit as it would seem to be from the positions of the commutator and brushes. The *actual* periods of short circuit are somewhat longer, owing to the conductivity of the spark, and last until the spark is blown out. The exact point of the revolution represented by Figs. 10, 14 and 18, is the point 130°, as marked below the curves. Figs. 11, 15 and 19 represent the positions of the brushes and commutator for the parallel periods

Fig. 7.



p_1, or, to give the exact position shown, 160°. Figs. 12, 16 and 20 represent the second short circuit period, s_2, or 190°, and Figs 13, 17 and 21 show the second periods of parallelism, p_2, or 220°. In all of these figures the relative positions of the coils and poles of the dynamo are also shown, the coils being numbered 1, 2, 3, just as they are on the dynamo. During the entire period represented by each four figures as above, the armature has turned through 120°, or one third of the revolution, and if we now consider the position of coil 2 changed to that of coil 1 in each figure, our figures will then represent the next third of the revolution. If coil 3 is supposed to occupy the position of coil 1, we shall then have the remaining third of the revolution.

It will be noticed that the more sudden and violent changes of potential occur just as the spark is forming and being broken at the front end of the commutator bar.

In examining these curves of potential it should be remembered that they represent the *difference* of *potential* at the ends of the coil and *not* the E. M. F. developed in the coil during the revolution. The sudden and marked changes in current which are shown at certain points of the current curves, are selected from a number of readings which showed great variation. In some cases the reading had an opposite value from that shown in the curve, and it seems probable, that all things considered, the average value of these points should be very little, if any, different from the others, and the whole should form a smooth curve without any very sudden changes. In Figs. 10 to 21, the arrow shows the direction of the current in each coil during that particular part of the revolution represented by the figure. In each of these figures a neutral line n n' is shown, in passing which the current in each coil becomes zero and reverses. It

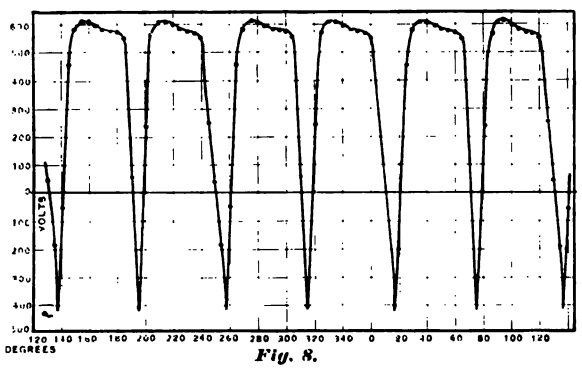


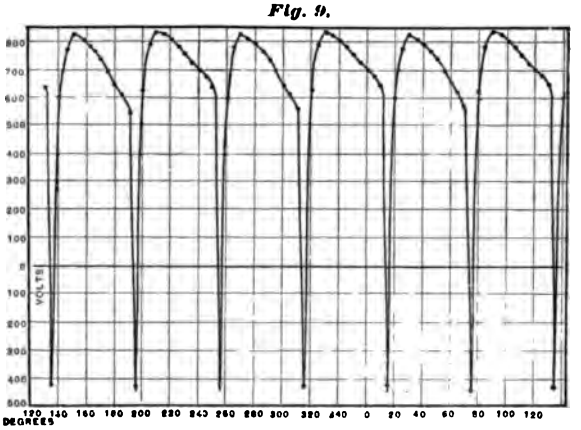
Fig. 8.

will be noticed that the direction of the currents in the various coils for the so-called periods of parallelism and short-circuit is quite different from what would be naturally expected and from what has been supposed to be the case in most of the descriptions of the action of this machine. For instance, in Fig. 11, we have the current in coil 1 flowing *toward* the junction of the three wires, showing that a current is flowing from the junction through coil 3 to the left pair of brushes and back through coil 1 to the junction again, or in other words that coils 1 and 3 are short-circuited upon each other. This state of affairs continues, with the light load, until commutator bar 3 touches the right following brush and short-circuit period s_2 commences. Thus it will be seen that there is no instant during the so-called parallel period when coils 1 and 3 are in parallel and that the *actual* parallelism does not occur until after the beginning of the period of

short-circuit. In the case of medium and full loads, however, it occurs at from 10° to 20° before the period of short-circuit. Another point worthy of notice is, that at the first instant of short-circuit, when the commutator bar just touches the following brush, there is no sudden increase of current as would be naturally expected, but the current in the short-circuited coils slowly and gradually increases until the short-circuit is broken by the blowing out of the spark, at which instant the current is a maximum in the coil whose commutator bar is opposite the sparking point, and in the other two coils it is about the same as the normal external current.

In Figs 7, 8 and 9 are shown curves of potential at the brushes, at the three different loads. These curves were obtained from the potential curves of Figs. 4, 5 and 6, by adding together the

Fig. 9.



potentials of the three coils according to their relative positions, and thus obtaining the potential at the brushes. It will be noticed that at all loads the curves fall below zero six times during each revolution, and that the difference in area and consequent effective E. M. F., is obtained more by the widening out of the curves than by increase in height. This is what we ought to expect from the change in duration of the short circuit periods. A comparison of these three curves of potential at the brushes, with the three curves of potential at the terminals of the dynamo will give one a good idea of the remarkable steadying power which magnets such as the fields of this dynamo, exert upon an intermittent current. No further explanation is needed of the fact that this dynamo will not work as a separately excited machine. The whole set of curves will serve to illustrate some of the very re-

markable effects of armature and field reactions and any one sufficiently interested to make a detailed study of them might possibly succeed in bringing out a number of curious and interesting, if not useful, results.

It is to be regretted that time did not permit the measurement of the exact electromotive force developed in a separate coil of the armature, as distinct from the difference of potential at the ends of the coil, and also the measurement of the changes of magnetism occurring in the fields of the dynamo. With the help of this additional data a very complete study of the internal actions of the machine might have been made, and possibly results might have been obtained which would throw useful light upon the interesting subject of armature reactions. The writer hopes at some time in the near future to make a further study of the machine in this respect and to present any further data obtained in this way as a supplement to this paper.

Cornell University, May 8, 1891.

TABLE I.

External Curves.

Degrees.	Light Load.		Medium Load.		Full Load.	
	Volts.	Amperes.	Volts.	Amperes.	Volts.	Amperes
120	166	6.8	362	6.8	561	6.8
130	162	7.0	328	6.5	546	6.3
140	150	6.2	308	5.9	516	5.9
150	165	6.9	337	6.7	561	6.6
160	172	7.0	362	7.0	573	7.2
170	176	7.4	360	7.4	570	7.4
180	168	6.8	352	7.0	563	6.8
190	158	6.6	316	6.4	526	6.3
200	146	6.1	294	6.0	509	5.9
210	168	6.9	336	6.9	561	6.8
220	178	7.4	352	7.0	571	7.4
230	176	7.2	360	7.1	566	7.0
240	162	6.6	350	7.0	553	6.6
250	160	6.3	308	6.3	524	6.1
260	150	6.1	305	6.2	516	5.9
270	168	6.8	336	6.7	558	6.8
280	178	7.2	350	7.0	568	7.2
290	170	7.0	354	7.1	566	7.0
300	162	6.8	352	7.0	561	6.8
310	160	6.5	313	6.4	540	6.3
320	148	6.2	308	5.8	505	5.9
330	168	6.9	334	6.5	554	6.6
340	176	7.4	347	7.0	571	7.4
350	174	7.4	352	7.2	568	7.2
0	166	6.8	352	7.1	565	6.8
10	162	6.7	326	6.4	536	6.6
20	148	6.1	308	6.0	495	5.7
30	168	6.8	342	6.8	546	6.6
40	176	7.2	360	7.2	563	7.0
50	178	7.4	365	7.3	568	7.2
60	168	6.9	362	7.1	561	6.8
70	160	6.6	345	6.7	546	6.3
80	152	6.3	305	5.9	514	5.9
90	162	6.6	336	6.3	546	6.8
100	176	7.3	360	7.0	568	7.2
110	174	7.2	365	7.2	568	7.2

TABLE II.

Internal Curves.

Degrees.	Light Load.		Medium Load.		Full Load.	
	Volts	Amperes.	Volts.	Amperes.	Volts	Amperes.
120	140+	.28—	13—	1.31	33—	
125	149	1.43	0	2.2	24	
130	160	2.56	170+	3.0	15	
135	173	3.71	190	3.25	500+	
140	268	4.28	549	15.0	212—	
145	500	30.0	107—	6.5	368	
150	180—	7.4	292	7.0	445	
155	290	7.0	374	7.2	503	
160	368	7.5	445	7.6	543	
165	409	7.5	490	7.85	580	
170	441	7.6	520	8.15	613	
175	464	7.7	549	8.5	627	
180	173	7.75	560	7.7	607	
185	190	9.2	555	7.3	582	
190	202	9.75	255	8.0	545	
195	190	10.5	130	9.3	37	
200	110	11.1	107	30.0	302	
205	190	26.0	206	12.1	305	
210	235	12.7	320	11.3	345	
215	202	12.0	295	11.1	263	
220	145	10.6	170	10.3	195	
225	65	10.0	105	9.22	137	
230	35	9.3	51	8.1	95	
235	3	8.5	24	7.1	70	
240	175+	7.75	9	6.3	40	
245	192	7.0	0	5.6	27	
250	198	6.5	181+	4.96	18	
255	178	6.0	50	4.25	0	
260	233	5.5	190	3.68	333+	
265	221	4.7	269	3.12	350	
270	195	4.0	237	2.7	303	
275	155	3.0	178	2.2	232	
280	120	2.3	80	1.5	170	
285	90	1.3	55	1.0	119	
290	35	.5	35	.2	76	
295	140—	.28	20	.5 +	51	
300	175—	.75+	6+	1.31+	33+	
305	184	1.25	174—	2.18	30	
310	190	1.75	195	3.06	15	
315	193	2.6	549	20.0	500—	
320	580	30.	150+	7.6	220+	
325	149+	7.2	304	7.0	390	
330	276	7.7	380	7.0	505	
335	362	7.6	450	7.3	555	
340	408	7.64	502	7.6	606	
345	437	7.78	535	7.85	641	
350	462	8.0	558	8.5	656	
355	173	8.6	566	8.6	652	
0	122	9.2	555	7.7	631	
5	190	9.9	256	7.3	610	
10	199	10.5	155	8.3	579	
15	149	11.4	107	30.0	74	
20	60	0	275	13.5	368	
25	241	13	347	12.2	407	
30	247	12.7	288	11.6	380	
35	199	12.3	224	11.2	285	
40	140	11.7	161	10.6	235	
45	83	10.8	107	9.22	167	
50	36	9.96	60	8.22	113	
55	9	8.94	35	7.1	59	
60	158—	8.25	14	6.3	39	
65	180	7.76	4	5.6	27	
70	197	7.41	187—	4.96	21	
75	174	7.03	180	4.25	77—	
80	142	6.45	83	3.68	331	
85	263	5.75	270	3.12	349	
90	230	5.0	274	2.7	316	
95	191	4.4	240	2.2	245	
100	127	3.22	186	1.5	165	
105	64	2.2	107	1.0	113	
110	23	1.61	61	.44	76	
115	9	.29	29	.5 —	43	

Discussion.[1]

Prof. Thomson :—If no one else cares to open the discussion I may make a few remarks. I called Mr. Thompson's attention to his statements in regard to the blowing out of the spark as expressing an erroneous idea in relation to the machine. This erroneous idea he will find expressed in Prof. Silvanus P. Thompson's Dynamo Electric Machinery, that the air blast in the Thomson-Houston three coil machine is for the purpose of blowing out the spark. That is not at all the case; it never was the case; and the air blast was introduced for no other purpose than to make it possible to run the machine steadily at high differences of potential with a commutator which could be freely oiled. It was found in the early experience with the machine that it was possible to construct a dynamo up to about twelve to fifteen arc lamps which would retain the normal working condition without flashing over, as it is called—short circuiting across the slots and the short circuit arc being carried around. But beyond that potential difference there was an eminent danger of its occuring, although a 25 light machine might run for an hour or an hour and a half without any flashing or short circuiting over. Of course the tendency to flash over, increased with the difference in potential. Now it was also desirable to run the commutator with a film of oil to freely oil the machine, and when the commutator was freely oiled the limiting potential which could exist without flashing was much lower. Even a 10 light machine might flash readily if there was a good deal of oil on the commutator. In the early days men would oil the commutator. It was almost impossible to keep them from taking an oil can and squirting on the commutator. The Brush machines were also very sensitive to this same action, and their recommendations were very distinct as to what was to be done to oil the commutator—that is, take a rag and grease it with a little vaseline and touch the commutator lightly. If the commutator was thoroughly oiled there would at once be a flash. Another thing, the oil on the commutator of course diminished the wear, so that if there was any little roughening it was from cutting and making copper dust around the relieved commutator. I tried in the early days to get rid of the difficulty and did get rid of it by using condensers on the machine. I took the three coils and segments. I led our connections from each segment to a triple condenser. That preceded the development of the air blast. There were three coils in inductive relation. On a machine up to 6 or 8 lights I found I could actually obliterate the sparking at the tips of the brushes and the brushes could be moved quite a distance without provoking sparks while the machine was carrying its load. But I also found that the condenser was very subject to perforation. There were conditions arising which raised the po-

1. By Messrs. E. Thomson, Rice, Geyer and Nichols.

tential enormously and I could under certain conditions get strong static discharges, (this is a very interesting matter indeed as looked at from the present standpoint in relation to alternating discharges.) With a coil of a single turn connected in the wire leading from the segment to the condenser coil, I could often get discharges of an eighth of an inch length snapping from one turn to the other; a discharge which refused to run around the coil—one in fact which went through the double insulation of wire and the air space between the turns instead of going a foot or two in the wire, and if it did not get a chance to do that, it was very apt to go through the condenser insulation. I tried to apply this principle to a much larger machine and I found I could not make the condenser live under those conditions. It would be perforated although it *was a remedy for the flashing.* The tips of the brushes were practically quiet. It did not fulfill the conditions in practice, so that it led to the trial of an air blast on the tips of the brushes, and the reasoning used in getting

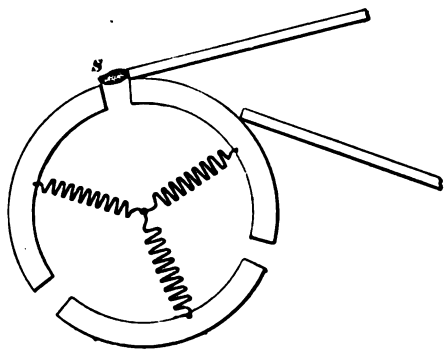


Fig. 1.

the air blast was this—to take a magnified image of the commutator brush and the slot (Fig. 1.)—that on the leaving of this segment there would be a small amount of current still fed from the segment to the brush, which would, as it were, tail off at *S* and the sparks would be thicker at the brush and gradually thin out to a fine point and then stop. Then the segment would change polarity and go on to the other side. But naturally if the change of polarity was immediate on the cessation of that spark there might remain, as it were, from the spark, hot gas which would enable the discharge to take place in the opposite direction, and that was a flash which would be carried over as an arc. My idea was simply to blow a cold blast of air—as it were to drive that spark away and insert, no matter how thin a layer of cold air before the reversal was accomplished and in that way to keep the insulation of the machine. That process is found effective even with a three coil commutator up to 3,500 or 4,000 volts, with the air pressure, of course, slightly increased as we rise.

Now a few words in relation to the three coil machine, as to its development, may be of interest. The reasoning which led to the machine originally was somewhat like this: Here we have the old Siemens type of machine—a single coil machine with two segments (Fig. 2). It occurred to us that by taking that coil and dividing it in the centre and putting in another coil and slipping this segment around and making room for another, we would have a continuously acting arrangement something like a three cylinder engine, and we would have, as it were, a triple coil (Fig. 3). I do not regard these coils at all as separate coils. They are not separate coils. The system is a system working as a single coil system. Now it is not at all surprising that any measurement of potential, taking between the branching point of the divisions or the common point and any segment should show considerable fluctuation because the joint is only a point in the combined coil circuit. The equi-potential points or points of zero potential may slip back and, as it were, fluctuate—pass into

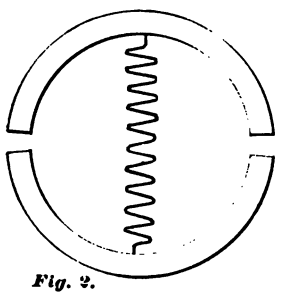


Fig. 2.

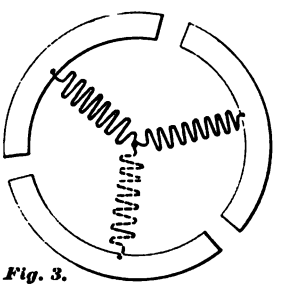


Fig. 3.

each coil, and do that a number of times in a revolution. Investigations show that it occurs twelve times. But what does it mean? It doesn't mean necessarily that the fluctuation is important. It means that the electrical condition of this junction of the three branches is changing several times in a revolution. The fact is that the activities of the coils are developed in nearly a sine curve, as I proved years ago—in fact in 1881—by the method of instantaneous contacts just then brought out. This was done by revolving a small, narrow segment and making contacts with a movable brush at successive positions. I got curves from the coils like that, and I got curves not only for the coils acting singly, but I got curves for the coils when acting together But it is proper to state that these curves, which were modified sine curves, were obtained with a machine working under what we may call now the disused condition of forward regulation. The three coil machine is capable of working in two ways. It may be regulated

by the commonly practiced method, or it may be regulated by forward regulation as we term it (Fig. 4). We may put a brush here (Fig. 4.) and another one at a moderate angle generally about thirty degrees, (according to the field strength of the machine this angle becomes less or greater). If the field is very strong we have to diminish this angle because the transfer of current from one coil to the other takes place more briskly. These brushes are yoked together and during regulation which was the original way in which the machine was worked these brushes were moved forward by the regulator to diminish the electromotive force gradually. It was possible to work this machine not only to cut out every light without a spark, but it was also possible where two machines were run in series as was common at that time to take one machine with its brushes at the position of full load, take the other machine and by carrying the

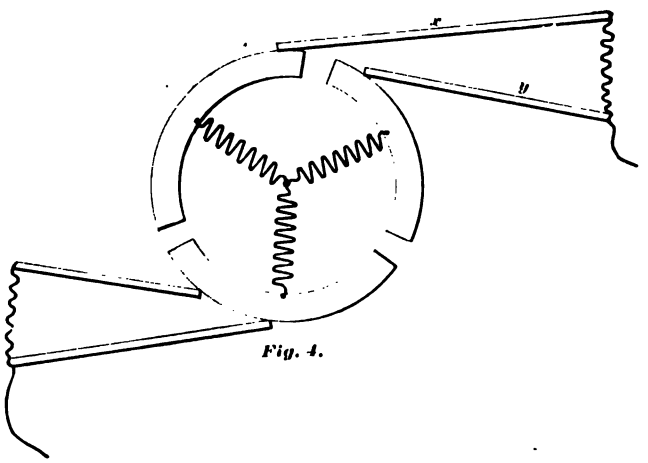


Fig. 4.

brushes around to actually convert it into a motor, so that every light in the circuit—say there were 20 lights—two ten light machines coupled in that way—every light could be taken out, and the two machines could be running, one feeding the other as a motor—the brushes having gone around without sparking to the opposite point—a very interesting thing indeed—and it was very curious to notice how the strain on the belts shifted as the transfers were made. While this method of operating the machine was undoubtedly a little more efficient than the present method; what we call the backward regulation, it did not give in all cases that kind of curve to the dynamo which was required for stability in the operation of all types of arc lamps. It was sometimes difficult to get the machines to be stable under some conditions, and even with changes in the proportioning, the quality of the iron would bother us with certain forms of lamps, although there were

many machines constructed and operated in that way, but we had to do a great deal more adjusting in the factory than we did after adopting the backward type of regulation. We got, in other words, by the backward regulation a greater droop in the characteristic curve at the normal current point—a tendency, in other words, for the machine to become of itself a constant current machine. No matter what character or regulation of lamps existed on the circuit, whether they were differential or simply shunt lamps, that machine would take care of them and handle them. The former machine was sensitive to the lamps and sensitive to the adjustment of the lamps—differential lamps working much better than simple shunt lamps of course. When the shunt lamp was adopted it required that the droop in the characteristic be much more sudden. These considerations will throw some light on the general question which has been opened for discussion. [Applause.]

DR. GEYER:—I have always thought that this machine is the most unique and interesting of all the machines built. I am very glad that a study of it has been made in this manner by Mr. Thompson. I should like to remark, however, that an exactly similar investigation was made as early as 1879 or 1880 on a Brush machine, which is one of the class called open coil. The apparatus was just the same, a ballistic galvanometer being used, but angles were taken only every ten degrees instead of every five degrees. The experiments were made more particularly to illustrate the action of any one particular coil or of two coils in series and finally of the machine as a whole.[1]

PROF. NICHOLS:—It is very late, but I cannot refrain from saying that I hope that Dr. Geyer will try to get these belated results on the Brush machine published. I think it would be a very interesting thing at this day, when the method of instantaneous contacts has reached a development and has an important and recognized field—to publish measurements which were made at that early day, that we might compare those results with what are being obtained now. It seems to me that it is a most unfortunate thing that these results were not published in full in 1879 or 1880. It appears that this work must have been done before the appearance of Joubert's well known paper, and it seems to me it would be very well worth while to resuscitate these old results and to give them in full.

MR. RICE:—In connection with the remark of Prof. Thomson, that the flashing of the commutator was due to the establishment of an arc across the segment through the hot gases, I think it would be interesting to state a fact which I believe is not generally known, but which we discovered at the works of the Thom-

1. Since taking part in this discussion, Dr. Geyer has met Prof. Thomas who participated in this investigation, and who has undertaken to prepare a paper for the Institute giving the details. For this reason Dr. Geyer's further remarks are omitted.—[*Editor.*

son Houston factory while experimenting with large arc lights for government use, using currents of from 50 to 100 amperes, the potential difference being 50 volts and run in connection with a machine that would produce about 75 volts. We found we could turn out the light and allow the arc to be completely extinguished for a full second; then switch on the current and the arc would be re-established. This was done repeatedly, showing that the hot gas was sufficiently conductive to re-establish the arc where there was a difference of potential which could not have exceeded 75 volts. The same phenomenon has been observed by us subsequently in arcs using but ten amperes, although with such a small current, the time during which the current can be shut off with re-establishment of the arc, is much less.

During all these experiments the carbons were maintained separated the normal arc distance, *i. e.* about one-sixteenth or three thirty-seconds of an inch for small currents, up to one-quarter or five-sixteenths of an inch for the larger currents.

Prof. Thomson:—I am glad attention has been called to that experiment for I have seen that case repeatedly in which you could turn off current by the switch and throw the current on again and the arc would re-establish itself, a complete dead space having existing between the carbons for some interval of time. I would say, calling attention to the paper of yesterday, that here we have an observation which goes along with those of the paper very prettily—that here we have just the case of the stoppage and re-establishment, and the experiments just mentioned prove that the alternations might have been very slow indeed and with a considerable interval of interruption in the case of large arcs, without permanent extinguishment.

The Chairman:—If there be no further discussion, we will return to the regular order of the programme, according to which the next paper is that of Dr. W. H. Wahl, of Philadelphia, on "The Future of the Aluminium Problem from the Chemical Standpoint."

Dr. Wahl:—When I set about to fulfill the promise that I gave some time ago to one of the officers of your society, I was confronted with the difficulty of presenting anything that was new upon the subject of aluminium. I found, for example, that within the past few months several admirable papers have been presented on the subject, which treated it in almost all its aspects very thoroughly and leaving really very little that was new to be said on those particular phases of the question. Prof. Richards, for example, in a lecture lately delivered before the Franklin Institute in Philadelphia, discussed the aluminium problem historically in a most admirable manner, giving a resume of the state of the art down to the present time, practically covering this phase of the subject. Mr. A. S. Brown of your society presented, I believe, only a few weeks ago an excellent paper covering substantially the same ground, and giving facts and figures covering certain improved processes, and in March of the

present year, I believe, Capt. Hunt, of Pittsburgh, read a most admirable and exhaustive paper on the subject before the Boston Society of Arts, in which he discussed very thoroughly and instructively the prospective uses of the metal, especially in connection with its engineering application—a paper to which I would call your particular attention. In casting about to find some new phases of the subject it occurred to me that while the electrical process had had a number of champions, there had been very little said concerning the possibilities of the restoration of the chemical processes in the production of the metal in the future, and thinking it might possibly be a meritorious deed to take the part of the under dog, I concluded to present to you some of the possibilities from the chemical standpoint of producing aluminium cheaply.

Dr. Wahl then read the following paper:

A paper read at the General Meeting of the American Institute of Electrical Engineers, New York, May 21st, 1891. Vice-President Crocker in the Chair.

THE FUTURE OF THE ALUMINIUM PROBLEM FROM THE CHEMICAL STANDPOINT.

BY DR. WM. H. WAHL.

MR. PRESIDENT AND GENTLEMEN OF THE INSTITUTE—I propose to invite your attention in the following remarks to the consideration of the possibility of cheapening the cost of producing aluminium by improvements in chemical processes.

It is obvious at the outset that the selection of the aluminious compound to serve for reduction, is of the highest importance. The compounds available for the purpose are three in number:

1. The halogen compounds (chloride, fluoride, etc.)
2. The sulphide.
3. The oxide.

Thus far, the compounds first named have been the only ones that have yielded satisfactory results on the commercial scale, and it would seem probable that if a substantial advance in the metallurgy of aluminium may reasonably be expected, it should be looked for in connection with these compounds that have heretofore proved to be the most pliable in the chemist's hands. It will be most instructive, therefore, to examine the processes based on the use of the halogen compounds of aluminium and to endeavor to ascertain to what extent the economics already realized in practice bar the way to further improvement.

These processes, which held the field alone until driven from it by the remarkable development of the electrical methods, are based upon the reduction of the chloride or fluoride of aluminium by means of sodium. For practical purposes it is found preferable to employ, in place of the simple chloride or fluoride of aluminium, the corresponding double salt of sodium.

First in order, let us consider the Deville-Castner process. This embraces three operations—the manufacture of the double chloride of sodium and aluminium, the manufacture of sodium, and the reduction of the double chloride by sodium.

The statement will not be questioned by chemists, that the use of the chloride as the aluminious compound to be reduced, is attended with difficulties. It cannot be produced in the wet way either by precipitation or by the evaporation of its aqueous solution, and a roundabout furnace process is the only method by which it has thus far been produced, commercially. Furthermore, it is so readily decomposed that it must carefully be guarded from the access of moisture. Also, though its reaction with sodium takes place with great energy, it has been found practically impossible to obtain by its use more than about 75 to 80 per cent. of the contained aluminium for physical reasons inseparable from the conditions under which the reaction occurs.

Its advantages are, its ready fusibility, in consequence of which the reaction with sodium begins at a comparatively low temperature, and the fact that the metal may be obtained entirely free from silicon and almost free from iron, the two elements that exert the most deleterious influence upon aluminium.

It is a just tribute to the skill and ingenuity displayed by the able chemists in charge of the Deville Castner plant, at Birmingham, to give them credit for having succeeded in reducing the cost of sodium-aluminium chloride from about 22½ cents (which is believed to be a fair estimate of its cost before the improvements of Webster and Castner) to about 6 cents per pound.

You will doubtless have heard much of the claims of several processes for the direct production of the chloride, by passing gaseous mixtures containing chlorine and carbon over heated alumina contained in retorts suitably connected with a condenser. By one of these described in Richard's admirable treatise on aluminium the cost of production is affirmed to be as low as 1½ cents per pound. By another process which is exceedingly plausible, hydrochloric acid vapors are passed into an electric furnace, in which alumina is being decomposed by carbon, the chloride formed being condensed in a suitably connected chamber.

Either of these plans, so far as one may form a judgment from their general features, appears to possess decided advantages over the older one employed in the Deville-Castner operations. Thus far, however, so far as I am aware, they have not been put

in practice on the commercial scale, and, promising as they appear to be, I incline to the belief that the fluorides present such advantages that if there shall prove to be any future for aluminium processes based on the use of sodium as the reducing agent, the fluoride compounds of aluminium and not the chlorides, will be used for the purpose.

The reasons for this opinion are: The fluorides are absolutely stable at common temperatures; they may be highly heated without suffering notable loss by volatilization, and, by employing proper precautions in bringing the reducing agent in contact with them, they can be made to yield very nearly the theoretical quantity of contained aluminium in condition to be at once and without further manipulation, alloyed, cast, etc.

Respecting the comparative cost of the chloride and fluoride of aluminium, while reliable data are wanting, I am confident that the opinion of chemists who have given the subject any consideration will incline in favor of the fluoride. There is this important advantage in its favor, that the resulting product from the reaction of sodium with aluminium fluoride, *i. e.*, sodium fluoride, by very simple and inexpensive operations, may be made to yield, on the one hand, sodium hydrate, which may be utilized for the production of sodium, and on the other hand, aluminium fluoride; thus regaining, at comparatively little cost, both the reducing agent and the aluminium compound to be reduced.

In the case of the chloride the end product of the reaction is sodium chloride, a substance of such little value as to be hardly worth the cost of evaporation and utilization. For these reasons, in the consideration of the future possibilities of the sodium process of producing aluminium, I believe the use of the chloride may safely be assumed to be impracticable.

We come now to the consideration of the remaining element of the chloride and fluoride reduction processes, namely, the question of the sodium.

I am aware that the weight of opinion among chemists who have given the subject very careful consideration is unfavorable to the future prospects of aluminium processes in which sodium is relied upon as the reducing agent. Richards, for example, whose opinion on all questions pertaining to the aluminium problem is entitled to respect, in a lecture on this subject lately delivered before the Franklin Institute, says on this point,

"I do not consider the sodium processes, however, as ever likely to take part again in the metallurgy of aluminium. I think their day is past." [1]

I will not venture to dispute the correctness of the conclusion to which this able and conservative author has arrived, since there are other metallurgical methods that appear to be much more promising. The author I have just quoted, indeed appears to be of this opinion, inasmuch as he says in the same paragraph, "..... although electrical processes are 'on top' at present and, I think, will stay there for some time to come, I am far from considering that they may not possibly be superseded by non-electric methods of reduction."

It may be true that the sodium processes have had their day, in the metallurgy of aluminium, but I am confident that the prospects for much cheaper sodium in the near future are decidedly brighter than those for cheaper aluminium. Without underestimating the great difficulties involved in the production of sodium, and duly appreciating the value of the services of Deville, Castner, Netto, and others who have contributed meritoriously to the problem of reducing the cost of its manufacture, I have been impressed of late with the belief that all the methods hitherto proposed and employed for the production of sodium are extremely crude in conception and imperfect in practice. For one, I think it would be entirely possible, very greatly to reduce the cost of making sodium by the metallurgical way, but to do so, it must be manifest to all who are familiar with the past and present methods of manufacture that these will have to give place to others operated on some radically different plan.

My reasons for this opinion are briefly these: The cost of a metallurgical operation is largely affected by the scale of magnitude on which it is conducted; and, where heat is required to bring about the needful reactions, by the economy with which it is applied to effect the result; and, if it be a furnace process, whether the operation is continuous or intermittent.

The capacity for production of a metallurgical plant increases in a much greater ratio than the item of labor cost; so that in such cases the doubling or quadrupling of the output may be accomplished with a correspondingly enlarged plant, with the aid of comparatively little additional labor.

Where heat is the prime element in effecting the reactions—

1 Richards. The Aluminium Problem. *Jour. Frank. Inst.*, March, 1891.

which is practically always the case—those who have experience with steam power plants need not be told how seriously the economies of a mill or factory are affected by the choice and proper installation of boilers and engines. In metallurgical operations the limits between economy and wastefulness in the application of fuel are much further apart. The question of economy, also, is materially affected by the manner in which the process is operated, *i. e.*, whether continuously or intermittently. A continuous furnace process, of which the blast furnace for producing iron affords us a typical example, realizes the highest attainable economy, and whenever it may be found practicable to adopt this feature of operation, the cost of the output will be notably reduced over processes worked discontinually.

Examining the sodium processes by the light of the foregoing statements, it will appear that none of them fully realize the conditions essential to the highest economy of operation. A few words of a critical nature upon the methods of Castner and Netto which may stand for the most advanced sodium processes, will enable me, I think, to make it clear, in spite of the decided advance over previous methods which they exhibit, that from the metallurgical point of view they are very imperfect.

Castner, for example, operates on a number of small charges, introduced into a group of steel crucibles about 18 inches wide and 24 inches high, and capable of receiving a charge of 15 lbs. of caustic soda and a suitable quantity of the reducing agent. The arrangements for removing a crucible from the furnace after its charge has been delivered, clearing it of the residuum, recharging and returning it to the furnace, permit these operations to be effected in about two minutes, so that the vessel still retains a good red heat when restored to its place for a second operation. The crucibles are heated by producer-gas.

The theoretical yield of a single charge of the above named quantity should be 8.92 pounds; the actual yield in current practice at the Oldbury Works, near Birmingham, according to Mr. J. Mac Tear, who has reported the results of a very thorough examination of the process, was 2.5 lbs. or 29 per cent. A much larger yield, approximating to the theoretical quantity has been obtained, but this cannot be realized in practice because the extremely high temperature demanded for it would result in the rapid destruction of the crucibles.

The great advantage of Mr. Castner's process[1] over older methods lies in the fact that, by the employment of a special reducing agent (an intimate mechanical mixture of coke and carbide of iron), he is enabled to operate upon caustic soda, which may be reduced by carbon at a much lower temperature than the carbonate. The inventor has claimed that by this process the cost of manufacturing sodium on the large scale will not exceed 25 cents per pound. Mr. Mac Tear has named as the result of his experimental work at the Oldbury establishment, 16½ cents (8¼d.) I have no data at hand from which to deturmine how nearly either of these estimates is actually realized in current practice. I can only say that the price at which the product of the works was, until recently, quoted on the market would yield the manufacturers 200 per cent. profit on the price named by the inventor.

Dr. Netto's process, in the opinion of competent judges, is the most economical method that has yet been put in practice, nevertheless, like that of Castner, it fails to realize save in one important feature, the conditions of highest economy that I have assumed to be essential in the metallurgical operation.

In order that the bearing of this criticism may appear, it will be well to present a brief description of the process for the information of those who may not be familiar with it.

Dr. Netto used a cast-iron retort, about three feet high and two feet in largest diameter, lined exteriorly with fire-clay, and placed vertically in a furnace in which it can be heated to the desired temperature. The flames of the furnace, after passing around the retort, may be directed laterally so as to pass beneath an iron pot supported above the retort and to one side of it.

This pot is supplied with caustic soda, kept in the state of fusion by the waste heat of the furnace, and provision is made for feeding the soda in a regulable stream into the retort at the proper time. In connection with the retort there is a condenser for the sodium, and also a trapped pipe at the bottom, through which the accumulated carbonate may be allowed to pass out of the retort.

The retort is charged at starting with 50 kilos (110 pounds) of charcoal or coke, and when this has been brought to bright incandescence, the valve controlling the access of the soda is opened,

1 Since the date of this paper, the author is informed that the process has been abandoned, and that a new process based on the electrolytic method is now in use at the Oldbury works.

and the fused alkali is permitted to flow into the retort and upon the glowing carbon. The reaction begins at once, and the sodium distilling over, is caught in the condensing chamber.

The feed of the alkali is controlled by an attendant who judges of the quantity to be admitted by the appearance of the flame of the gases, which burn at the outer orifice of the condenser. The process goes on continuously until the charge of carbon in the retort is nearly spent, the heat abstracted from it as the reaction proceeds, being restored by the heat supplied to the exterior of the retort from the furnace, which is kept constantly fired.

When the carbon in the retort shows signs of exhaustion, the flow of soda is checked, the retort is charged afresh with charcoal, and when this has been brought up to the right heat the operations just described are repeated.

In Netto's process, as in Castner's, it is found necessary, in order to avoid the rapid destruction of the retorts, to maintain the heat of the retort and contents only slightly above that required for the decomposition of sodium hydrate by carbon, which requires only a full red heat to deliver sodium freely, and has the economic advantage of permitting the use of crucibles or retorts of cast-iron. The reaction of sodium hydrate, however, as I have already intimated in the consideration of Castner's process, is not complete, but, according to the heat of the charge, and to some extent, perhaps, by the manner in which the component parts of the charge are brought into contact, a varying proportion of the hydrate is always converted into carbonate, which can only be decomposed at a temperature approximating that of white heat.

In Castner's process the yield of soda, as we learned, is about 30 per cent. of the theoretical; in Netto's it amounts, according to the most reliable data I have at hand, only to 20 per cent. It appears, therefore, that 80 per cent. of the sodium charged into the retort in the form of caustic soda, is converted into carbonate. This carbonate, after the operation of the retort is once established, is permitted to discharge itself automatically through a siphon, provided at the bottom, and, after re-conversion into caustic by treatment with lime, is ready to serve for another operation.

The advantage of Netto's process, as compared with Castner's lies in the fact that the former is continuous—at least approximately so—a feature which permits of a large saving in the cost of

labor for the same amount of output, since it dispenses with the frequent withdrawal, cleaning out, recharging and returning of crucibles to the furnace, incidental to the latter; furthermore, the conditions under which the Netto process is operated are, other things being equal, more favorable to the economical use of the fuel. Nevertheless, so thoroughly is every detail of the Castner process carried into practice, that the cost of production by the two processes is probably almost the same. I have no precise information as to the actual cost of sodium by Netto's process, but, if the selling price may be considered as a reliable indication, the opinion just expressed is substantially correct.

It may be unwarranted, in the face of what has already been accomplished in this branch of the chemical industry, to anticipate still further and greater advances in the direction of diminished cost of production, but the recent history of aluminium should prove a warning to him who may be inclined to play the role of Sir Oracle; and this warning applies, in my judgment, with special force to the sodium problem, as I will endeavor to show.

As comparison is the most instructive form of illustration, I will ask your attention to the following parallel, by which I wish to exhibit how very wasteful the present representative processes for sodium appear; and also, by comparison with other metallurgical processes, how far we should be able to improve upon them by the intelligent application of well-known principles; to wit:

Starting from the reaction which most clearly represents what takes place in the retorts of Castner and Netto, namely:

$$3Na\ O\ H + C = Na^2\ C\ O^3 + 3\ H + Na,$$

and assuming that the partial conversion into carbonate of the sodium contained in the caustic and the liberation of only $\frac{1}{3}$ of the metal at one operation, afford practical advantages on account of the moderate temperature at which the reactions take place, the following conclusions may be stated.

The molecular weight of the 3 $NaOH$ in the preceding formula is 120; that of the carbon 12 (the ratio between them being as 10 : 1.) Assuming that the heat necessary to cause the foregoing reactions to take place, has been supplied, one pound of carbon interacting with 10 pounds of caustic soda (containing 5.75 pounds metallic sodium) should yield:

1.92 pounds sodium metal and

3.83 pounds sodium in the form of carbonate.

(The value of this carbonate should properly be credited by deducting it, less the cost of re-converting into caustic, from the cost of raw material. This credit will appear later.)

As that portion of the reaction which results in the production of the carbonate is exothermic (*i. e.*, one which liberates heat), its influence on the question of the heat involved may be ignored.

One pound of carbon at the temperature of reduction yields in round numbers two pounds of sodium. To this must be added the carbon necessary to heat the retort and its contents to the reduction temperature, and to maintain it at that temperature while the reaction is taking place The conditions of practice are so complex as to render any theoretical estimate of this factor of no value. We shall be quite safe, however, in accepting as a guide the figures actually obtained in practice, in the reduction of another oxide with carbon, *i. e.*, the oxide of iron, the data concerning which have been so carefully observed that we can hardly go astray.

In modern blast furnace practice one pound of carbon suffices for the reduction of one pound of iron. The reactions in the iron furnace may be explained by assuming, first, the formation of carbonic oxide by the union of the carbon with the oxygen of the blast, thus:

$$3\ C + 3\ O = 3CO;$$

and the formation of metallic iron by the interaction of this carbonic oxide with the ore, thus:

$$Fe_2\ O_3 + 3CO = Fe_2 + 3CO_2.$$

Whether this expresses correctly the course of the reactions within the blast furnace, or, whether the reduction of the oxide of iron is effected directly by the carbon, is a matter of indifference for the purpose of my illustration, since the final result, *i. e.*, the reduction of a metallic oxide by carbon with the aid of artificial heat is the same in the case of both iron and sodium.

Now, proceeding with the iron reaction, as I have previously done with sodium, theory requires that 36 pounds of carbon at a certain heat shall reduce 160 pounds of ferric oxide, yielding 112 pounds of metallic iron. One pound of carbon should therefore yield, say, 3 pounds of iron. Now, one pound of carbon, it will be remembered, actually suffices for the reduction of one pound of metallic iron, or approximately one-third of the amount of metallic iron which theory demands.

It follows, therefore, that for every pound of carbon usefully consumed in the reduction process, two pounds of carbon are consumed in heating the charge of the furnace to the temperature at which the reaction between the residual carbon and the ferric oxide will take place.

The comparison between the economy realized in the production, respectively, of iron and sodium, may now be instituted with instructive results.

As previously pointed out, theory demands that one pound of carbon at the temperature of reduction shall yield from caustic soda, two pounds of sodium.

From the figures of fuel consumption in the published accounts of Castner's and Netto's processes, I have been able to make the following estimate, viz:

Castner obtains one pound of sodium with a consumption of 14 pounds of fuel.

Netto obtains one pound of sodium with a consumption of 11.6 pounds of fuel.

Averaging these results, it will appear that one pound of carbon yields in practice approximately one-twelfth of a pound of sodium where theory demands two pounds. The conclusion is at hand, therefore, that of the 12 pounds of carbon required for one pound of sodium $\frac{23}{24}$ths, (or 11.5 pounds), are consumed in the work of heating and maintaining the retort and its contents to the temperature of reduction.

The iron furnace, as previously calculated, requires but two pounds of fuel for this work, therefore, the latter is approximately six times as economical in respect of heat utilized, as the sodium furnace.

The comparison, I believe, is a fair one, and the conclusion is justified therefrom that, by the application of heat with the same economy is realized in blast furnace practice, the cost of sodium could be reduced to about one-fourth of the present figures.

Taking Mac Tear's estimate of the cost of the Castner process, namely, 8¼ d. (16½ cents,) and allowing 2½ cents per pound as a standing charge for the caustic, the fuel saving indicated above would bring the cost of the metal to the very low figure of 5 cents per pound.

Assuming, now, that we operate on an aluminium fluoride with the cheap sodium just obtained, the final products will be aluminium metal and sodium fluoride. From the latter product,

by a process like that of Herr Grabau of Hanover, or by several other available processes analogous to it in general features, the aluminium fluoride almost chemically pure, may readily be regenerated, thus offering the material for the continued operation of the reducing process, and at a cost, I feel assured, that will compare favorably with that of anhydrous alumina prepared from beauxite, which is the source of the aluminium manufactured by the electrolytic methods of Hall, Minet and Herault.

Furthermore, a simple series of reactions could readily be made available, by which not only the fluoride would be regenerated in the form of aluminium fluoride, but the sodium also would be recovered in the form of caustic soda, which last would furnish the raw material for the production of more sodium.

Allowing for the unavoidable losses in operations of this kind conducted on the large scale, it is not an extravagant estimate to assume that 75 per cent. of the sodium could thus be recovered after it had served the purpose of reacting on aluminium fluoride.

The regeneration of the sodium by some such method as I have here outlined, I need scarcely add, would mean another notable reduction in the cost of sodium metal, since it would mean supplying a large proportion of the raw material needful in its manufacture at perhaps one-half of its market price.

Assuming that this item of saving represents one-fourth of the cost of manufacture, the figure previously named (5 cents) would be reduced to $3\frac{3}{4}$ cents.

The realization of these economic conditions on the industrial scale, of the feasibility of which I feel assured, would place the metallurgical methods again in a position to compete advantageously with the electric.

On the basis of these facts and inferences let me present you with an interesting comparison.

Capt. Alfred E. Hunt, in a lecture recently delivered before the Boston Society of Arts, gave an estimate of the probable cost of manufacturing aluminium by the electrolytic method practiced by the Pittsburgh Reduction Company, of which he is the head. This estimate is based on the assumption that the manufacture shall be conducted on a scale of much greater magnitude than at present, and with the cheapest source of power, namely, water power.

Apropos to this estimate of Capt. Hunt, I wish to remark that it has afforded me a most interesting subject for study, and I de-

sire here to declare my conviction that it is in no single item exaggerated on the side of economy. Respecting one of the figures named—that of the cost of water power—I entertained at first some doubts as to its accuracy, and to satisfy myself on the point I presented the inquiry to my friend, Prof. Coleman Sellers, an engineer of wide experience and at present one of the Board of Consulting Engineers to the Cataract Construction Co., which is engaged in the important work of utilizing on a grand scale the water power of Niagara Falls.

I will take the liberty of quoting to you from a letter which I received from him some data covering the subject of my question:

"Almost all the existing examples of water power are dependent upon accumulation of water in reservoirs created by erecting dams across existing rivers and so accumulating water and distributing it by canal to the water wheels, as at Holyoke, Lawrence and our own Schuylkill River. The water power, for instance, of the Schuylkill and sold by the Schuylkill Navigation Co., was quite limited in its amount and is already exhausted and it may be assumed to be somewhere in the neighborhood of $30 or $35 per horse power per annum, for day use only.

"These water powers are generally sold either by day use or by 24 hour use, and for the purpose of metallurgical reductions it would probably be required on the 24 hour basis.

"The difficulty about getting at the cost of these water powers is the uncertainty of the continuance of the dams that are built to retain the water. These are liable to decay and deterioration and sudden freshets often destroy them entirely, involving heavy costs that are not known until the event occurs. The exceptional case of Niagara presents an absolutely unfailing water power that does not vary the year round. The difficulty in the way of its use, however, consists in the extreme width of the breast of the dam and the necessity for carrying the water through a long surface canal to the lower chasm where it is to be used, or the digging of the tunnel as a tail-race to carry away the water that has been used from the wheels or other motors.

"A canal was built about 40 years ago which is still in use, that carries about 6,000 horse power to the lower river, but this amount of power has already been more than exhausted by users who availed themselves of it, and its further extension by that method would be very costly.

"The present scheme of utilizing Niagara Falls by the company in which I am interested, is by means of a tail-race tunnel under the town of Niagara and the preliminary work now being done, when completed, in about a year, will throw into the market about 120,000 horse power that may be said to be absolutely permanent and endurable, and capable of being worked for 24 hours in the day.

"It is premature to say what this power will bring in the market, but it is the only example I know of, where water power can be counted on as absolutely stable and not liable to any disturbance or delay and where the price will be comparatively low.

"I should think that you might safely say that it presents the possibility of power being obtained at less than half of what it would cost in any other known case, taking into consideration all contingencies. Small amounts of power may be obtained from streams of water of a reasonable degree of stability and far away from existing markets as low probably as $4 or $5 per horse power per annum, but this is subject to the risk before mentioned.

"It is not alone the mere cost of water power that must be considered, but in a case like this it is the continuance or permanency of the power that will give it its value, and that power will be the most valuable that is liable to the least fluctuation either in quantity or cost.

"I do not think, however, that it will be safe for you to predicate the value of metal produced upon anything short of about $15 per horse power per annum, for 24 hour power, and that in very large quantities, while perhaps $20 per horse power per annum would cover all contingencies."

Taking the estimate of $20 per horse power per annum as sufficient "to cover all contingencies," we would have as the cost of one horse power per hour 0.238 cents (or say about one-fourth of a cent.)

This would give for 22 horse power 5¼ cents, and for 22 electrical horse power, allowing 10 per cent. for loss in the conversion, six cents. Capt. Hunt allows five cents for his power, which, considering that the figure named by Prof. Sellers is obviously intended to be liberal, I feel satisfied is a fair one. This item of Capt. Hunt's estimate is the only one respecting which I have at any time entertained any doubts, and I have been so impressed in reading his admirable lecture, with the carefulness and accuracy

of his statements and his evident desire to avoid any suspicion of exaggeration, that I am quite prepared to accept the other figures which are based on the experience gained in the practical working of his company's plant.

Resuming the thread of my discourse after this slight digression, I beg your attention to the parallel to which I alluded. Capt. Hunt's estimate of the capabilities of the electrolytic method of producing aluminium on the largest scale is as follows:

BY THE ELECTROLYTIC METHOD.

COST OF PRODUCING ONE POUND OF ALUMINIUM.

2 lbs. alumina (contains 52.94 per cent. Al_2) at 3 cents	.06
1 lb. carbon electrodes, at 2 cents	.02
Chemicals, carbon dust and pots	.01
22 E. H. P. for 1 hour (water power)	.05
Labor and superintendence	.03
General expense, interest and repairs	.03
	$0.20

In presenting, by way of comparison, the possible figures of a metallurgical method, conducted on the same extensive scale and on the lines of my previous comments and explanations, I will assume that that the cost of the last two items of the foregoing estimate will be the same, an assumption which may be made with safety.

Referring now to the preceding data, we will have

BY THE METALLURGICAL METHOD.

COST OF PRODUCING ONE POUND OF ALUMINIUM.

3¼ lbs. $Al_2 F_6$ (containing 32.7 per cent. Al_2), at 3 cents	.09¾
2¼ lbs. sodium, at 3¾ cents	.09⅜
Labor and superintendence	.03
General expense, interest and repairs	.03
Total	$0.25⅛

I think that I have presented in the foregoing, the capabilities of the sodium process within entirely reasonable bounds. I admit, however, that I have made the best possible showing for the metallurgical side, except in respect of the last two items, in connection with which, metallurgical operations should show an appreciable advantage over the electrolytic and may bring the totals of the two estimates practically to an equality.

The second available compound named as a possible source of aluminium by chemical methods is the sulphide, including therein, of course, the double sulphide with sodium or other alkaline

metal. I refer to it only because of the fact that its use for the purpose was originally suggested by one of the ablest chemical engineers of our day, the late Mr. Walter Weldon. Thus far, however, nothing has been accomplished with it, and though it may possibly be made to serve to advantage in electrolytic methods, I am satisfied that the probabilities are very slight indeed, that it will ever prove sufficiently useful as a source of aluminium, to be able to compete with other chemical methods.

Not only is its manufacture attended with unusual difficulties, but its reduction also, so far as I am able to judge from personal experience, cannot be affected as readily as that of the chlorides and fluorides, and the resulting product is much less pure than that obtained from the other aluminous compounds just named.

The sulphide, I believe, is much better adapted for the production of aluminium alloys than for aluminium, but the difficulty of obtaining the product free from serious contamination is probably the reason why the sulphide method has never made a record for itself.

I consider the case of the sulphide from the metallurgical point of view, to be so impracticable as to be hopeless.

The third aluminium compound, which the metallurgist may hope to make available as a source of cheaper aluminium, is the oxide. Considering the subject from the chemical standpoint, I am persuaded that the attack upon the problem, with this compound as a starting point, offers even greater possibilities of ultimate success than the sodium-and-fluoride method we have just passed over, and for the reasons, that it involves the possible application of the cheapest of all reducing agents—carbon—in the production of the metal, and the employment of much more direct methods and simpler and less expensive apparatus than is demanded by the method with sodium.

The fundamental fact that alumina can be reduced to the metallic state by carbon, is established. It is proved beyond peradventure by the operation of the electric furnace of the Cowles Bros., in which heat, developed by the transformation of electric energy upon the passage of the electric current through a fragmental resistance material of carbon and alumina (with copper or other metal present as a solvent), is known to be the principal agency in effecting the reduction. The same action most probably takes place, to some extent, in the operation of the Herault furnace, and certainly does so, if the statement can be verified

that the output of this furnace is greater than can be accounted for by simple electrolytic action.

The reduction of alumina by carbon, therefore, may safely be stated to be simply a problem of temperature, and this temperature is probably not far above the fusion point of alumina.

This, unfortunately for the practicability of the direct reduction process lies enormously high, so high, indeed, that it does not admit of instrumental measurement, but can only be approximately estimated as somewhere between 2000° and 2600° *C.*; a point which cannot be reached save by the intervention of electric heating.

There is an alternative, however, that offers a ray of hope to the chemist. There are other methods besides that of fusion for obtaining alumina in the state of a liquid. We have, at least there are very good reasons to believe that we have, solvents for alumina, and if the condition of fluidity is as important a factor as it is believed to be, in inducing chemical reactions, we have the possibility open to us of establishing conditions favorable to the reduction of alumina by carbon, at temperatures perhaps far below the fusion point of alumina.

The fluorides of the alkalies and the alkaline earths, we have reason to believe, dissolve alumina freely, and there are other compounds into which alumina enters that may be dissolved in the proper menstrua.

Here is the vulnerable spot in the armor with which that intractable substance is surrounded, and by concentrating his attack upon it the chemist may yet succeed in piercing it. Whether by this mode of attack he will be able to produce aluminium directly is gravely doubtful in the light of our present experience. I can only affirm my belief that the thing is within the range of possibility. On another point, however, I can speak more confidently, namely, as to to the practicability of producing the alloys of aluminium by some such method. Of this I have not the slightest doubt. I will go even farther, and venture a prediction that in the near future the alloys of aluminium will be produced at least as cheaply by chemical methods as by the present electrical methods.

I have now passed over the ground, though hastily, that I had mapped out for my discourse upon this most fascinating technological problem. I have thought that by presenting the aspects of the problem from the chemist's standpoint the subject could

be made more interesting to a representative body of electricians, such as the one I have the honor of addressing.

Briefly summarized the case stands thus:

The processes in which electricity is applied, either for the direct reduction of aluminium by electrolysis or as the agency for producing the heat required to effect its reduction by chemical reactions, have outstripped the metallurgical methods in the race for supremacy and have taken a decided lead.

Within the past two years the metallurgical processes as represented by the improved methods of Castner, Netto, Grabau, and others of less renown, have been forced to abandon the field to the electrical methods of Hall, Minet, Cowles and Herault.

In one brief lustrum to have overtaken and gained the mastery —though it should prove to be but temporary—over a champion so redoubtable as the chemist, who has been striving for the glittering prize through half a century of toilsome progress and indifferent success, is an achievement of which the electrician may well be proud. It behooves him though to look well to his freshly-earned laurels, for his victory will stimulate his rival to renewed exertions.

But let our sympathies incline this side or that, if the outcome shall be—and I am persuaded it will speedily be—the final solution of the aluminium problem, we may rejoice in common, for in the victories of peace, the vanquished share with the victors in the spoils of the battle.

I thank you, Mr. President, and Gentlemen of the Institute, for your attention.

Discussion.[1]

Prof. Thomson:—I am sure the thanks of the Institute are due Dr. Wahl for the admirable exposition of the aluminium question from the chemist's side. I have listened with the greatest interest to the statements and arguments put forward, and I do hope indeed that the chemist will come to the top, even to the sacrifice of the electrical part of the question. There is a source of heat, it has occurred to me during the reading of the paper, which has been neglected and which might probably be turned to account in this direction, and that is the mere concentration of the sun's heat by large mirrors and lenses. The temperature is, of course, almost that of the sun itself and will certainly fuse alu-

1. By Messrs. Thomson, Crocker, Vander Weyde, Waldo, Weston and Oberlin Smith.

mina, and we will get it at simply the cost of the use of the apparatus on a sunny day. This is simply an idea which now occurs and which may, of course, be shown to be fallacious, but at the same time I can imagine an enormously large lens or mirror which brings its rays to a focus at the bottom, as it were, of a deep crucible, so as to have our material there subjected to that cheapest heat of the sun from which all our heat is derived through a series of changes which involve great losses.

THE CHAIRMAN :—In this connection I would say that I happen myself to have heard of such a process. A gentleman told me that he was building such an apparatus. I do not know how far he has gone, or whether he has any results. But he certainly intended at any rate to put that method into practical use. There is no doubt about the possibilities of high temperature and its cheapness. By such a method I think myself that there is absolutely no question about the possibility of reducing aluminium by carbon alone provided we have the temperature. I think the argument that carbon has insufficient chemical affinity to draw oxygen away from aluminium is fallacious because in the first place, as Dr. Wahl says, the Cowles process is a proof that sufficient temperature will reduce aluminium, because I do not consider that the action there is electrolytic at all. In the second place, carbon can reduce sodium and potassium, which are similar in their affinities to aluminium. It seems to be simply a question of temperature. Carbon has the peculiar property of developing high relative chemical affinity for oxygen at high temperatures. If temperature and carbon are all that are needed, the sun and ordinary carbonaceous material will certainly make a very cheap process for reducing aluminium. But the production of that temperature by anything but electricity or the sun's heat seems to me very unlikely. No other temperatures reach near that point nor is there any likelihood of their reaching it. Regenerative furnaces produce very high temperatures, but not high enough for this purpose. Of course, the principle of regenerative furnaces might allow us to reach very high temperatures, but I doubt if they would be sufficient.

DR. WAHL :—In connection with this portion of the discussion I desire to say that while I have no doubt that the reaction in the Cowles furnace is to some extent an electrolytic action, I am of the opinion that it is principally what I have described it to be (and what I believe it is generally accepted to be)—a reduction by the excessive heat which can be produced in the electric furnace—purely a chemical reaction effected by the enormous heat generated by the passage of the current through the resistance material of the charge. But what actually takes place in the Cowles furnace or in the Herault furnace it would be hazardous to affirm. I would not wish to have myself quoted as making any positive statement. The conditions existing therein must be exceedingly variable. We may interrupt the operation mid-

way and find one state of things, and stop at another stage of the operation and find something quite different. It is probably safe to describe the operation as partly electrolytic and partly chemical. As to the possibility of obtaining a sufficiently high temperature, directly, with the aid of electric heating, I think it is impossible.

Dr. Vander Weyde:—It was suggested to use the direct rays of the sun. I wish to remark, however, that the direct rays of the sun are not at our disposal always. If we use electricity it is always at our disposal and where does it come from? From the fuel which we owe to the sun. It is only a roundabout way of getting it from the sun. Electricity we owe to the sun.

Dr Waldo:—I suppose that I might fairly be put in the opposition. I am one of those unfortunate individuals that have to use aluminium I am very much reminded of a remark that a lady friend made to me, after I had gotten off an essay on the present delightful condition of the world. She said, "Dr. Waldo, I am not so much concerned with the world as it is, as with the world as it ought to be." In regard to aluminium, I began my contemplation of the problem some three years ago, and I have been contemplating it pretty steadily ever since, and the amount of misinformation respecting the properties of impure aluminium which has been printed has made me tired. There are certain conditions of shop practice which belong to that perception of the human mind which goes centuries ahead of the actual explanations of science. No one ever supposed that Stradivarius knew why he built his violins so that they would give tone pictures that would last for all time. But Stradivarius had a certain shop knowledge, and it took two centuries to produce a Helmholtz to give a scientific basis for that shop knowledge. Aluminium is a material of structural engineering and the conditions of purity to which it must conform, are based on those ideas which belong to the shop and seldom belong to the laboratory. If our mills accept the specifications from men who know about copper wire we are required to guarantee that the copper shall have a conductivity of 98 per cent If we accept the specification for parts of machines of electrical engineers, we require that they shall have certain physical peculiarities and the materials which go into our work are sent to the laboratory, and we find that our Banca tin is 99.9 and 99.92 per cent. pure. We find our copper is 99.8 and 99.9 per cent. pure. We find that the zinc we use has nearly the same high purity. Our new works were built at the beginning of the aluminium era and we did not stop to think that aluminum was aluminium and we bought freely. We bought many thousands of dollars worth of metal. Then we began to learn our lessons afterwards. The books and papers from the laboratories said nothing was simpler than to cast aluminium bronze. We proceeded to do that very simple thing of casting aluminium bronze, and we poured more

aluminium bronze than any concern in the United States. We poured it systematically and got uniformly unsatisfactory results. Here are some of the early results. [Showing some samples.] Here is a section sawed right out of the inside of an experimental propeller wheel. We expected to get at least 60,000 pounds to the square inch on a direct pull. It was poured carefully as the books direct—bottom poured as the books direct and that was the result. Nobody knew why. One of the head casters thought the metal was poured too hot, and another thought it was poured too cold, and another said it was not poured right. [Laughter.] But as the result, we began to learn about aluminium and we discovered that there is one thing necessary; that is, that the experience of past centuries cannot be safely ignored, because aluminium is a new metal; that if you put silicon and put iron and put all the bye-products of a furnace into your aluminium, you will get an unsatisfactory result every time

Now the condition of the aluminium problem is this: we have two leading corporations in the United States producing aluminium. The United States government with a sharp eye to the encouragement of all new industries has promptly put a tariff on aluminium. The two companies immediately get into lawsuits, and a judge of the Court decides what the price of aluminium shall be.

It takes a bright man to predict that a new process is going to be discovered; so that in the present condition of affairs, I do not know that the estimates that we have listened to are of very great immediate value to us. All of the processes which depend on the chlorides, depend also on the cost of acids in this country as compared with their cost in England, and while the estimates we have heard are quite correct so far as the cost of steam and that sort of thing is concerned, the price is likely to be maintained relatively high in the United States. But we would not care so much for the price if we had the quality. Here, [showing] is a bar broken at the United States Watertown Arsenal machine, which originally was just half this length. It is made of a nice aluminium, and an aluminium of over 99 per cent. purity. It is fatigued the whole length. It behaves just as the material you and I want to use in structural engineering should behave. The same percentage, high in silicon, gives this result [showing]—broken short off; a small expansion from breakage strains and the metal generally short and sharp for use. Here are as large sections as I could bear to bring down here of heavy castings where the metal has been turned and sawed, in order to show the structure of the metal. That is a 10 per cent. bronze which, you see, is perfectly solid [showing a solid section of 24 square inches]. A part of the section is left here entire that the section may be shown out of which the whole of the metal is made. That, of course, is the king of bronzes. There is no known material, except steel of extraordinarily good quality, which will give you the economical values aluminium bronze gives you.

Here is the same style of casting, but showing the effect of improper work in the casting of the metal in the blow-holes which come under the risers. This metal is perfectly sound otherwise, but under the risers are these blow-holes, which unfit it for the particular work for which this was designed. But a properly formed riser gives you a texture of this nature [showing a solid casting with different riser].

These results are extremely important, because under a proper manipulation of the bronze it is possible not only to give metal castings which are absolutely non-porous, which will stand any pressure up to several thousand pounds to the square inch but it is possible to give them strengths running from 90,000 to 100,000 per square inch in the casting. But I have taken pains to come this afternoon because some one from the standpoint of the engineer must put on record a distinct and emphatic protest against the use of impure commercial aluminium. It simply cannot be used to get good results I mean it cannot be used in our practice, and its systematic advertising and use, results only in a detriment to the whole aluminium industry.

Now to come to the important application of aluminium bronze. Perfectly pure aluminium, as you quite well know, probably has a conductivity equal to copper, bulk for bulk and it has three times the conductivity of copper, weight for weight. I am speaking now of aluminium which is free from silicon and from iron. *A priori*, therefore, it is the best known metal for alloying with copper in order to give copper the structural strength necessary for all forms of electrical engineering. But here the impurities of silicon and iron come in with a particularly damaging effect. The conductivity of the alloys of aluminium and copper as ordinarily made, bear no relation whatever to the proportional bulks of the metals themselves. I cannot say much on this question, because it is a question which is being very carefully investigated in our own laboratory. But that is true, and until the producers of aluminium bring the aluminium product over 98, 99 or 99.9—the nearer they can come to it—the electrical engineers have got to be content with quite a low conductivity, both in all sorts of places where metal is applied, and in their wires where they want strength, and expect to get the conductivity, but fail. The present position of the aluminium bronze is very closely akin to the early years of steel. I heard a question asked of a famous chemist as to whether he could determine the carbon in steel. He said: "Do you really mean that?" "I do," was the answer. The chemist said: "I cannot." Now the analysis of copper is in its infancy. I doubt whether any accurate man would rely on the determination of copper. But the moment you add aluminium and silicon to copper the question is immensely complicated. The first real work, therefore—now that we have a supply of reasonably pure aluminium, or that we can expect to get a supply of reasonably pure aluminium—is to be done on the scientific

side of the question. We have got to have accurate methods of analysis, and we have got to have a great deal of work done on the mixture of copper and pure aluminium, and of aluminiumas we get it chemically united with silicon and with iron.

The color of the bronze is a royal color [showing specimens]. It is merely dipped in nitric acid to get out all the color, and you will get an idea from the color on dipping it, what the strength and general value of the metal is.

I think, Mr. Chairman, that is all the comment I have to make. [Applause].

THE CHAIRMAN:—In regard to the conductivity of the alloy, I should doubt very much if the average conductivity of its constituents, or any approximation to it could be expected. For example, pure silver put into copper will very seriously interfere with the conductivity, and pure silver is rated better than copper in conductivity. That seems to be a fatal fact about the conductivity of alloys, that alloying usually reduces conductivity very seriously, and the smallest percentage of alloy or impurity—whichever you may call it—has a very great effect in reducing the conductivity.

DR. WALDO:—Of course, we are all familiar with the results pronounced by Dr. Matthiessen—that metals are divided into three groups; the first, where the conductivity of the alloy is wholly different to that which would come from the relative volumes of the metals themselves; and the second class where the alloys are exactly proportional in their conductivity to these volumes; but that only enforces the remark I just made. Those results of Dr. Matthiessen are dependent on early and not very precise methods of analysis of the metals used. He neither knew the purity of his copper nor the reactions which take place in the melts, and the results he gave are to be held tentatively for the present. My own belief in the matter is somewhat different, but it is founded on small experimental attempts, and I am not yet prepared to put these in shape for discussion. In discussing all these alloys, the best conception to have is, that the alloy is a glass rod transparent to electricity. The oxides and other things which come in and which are precipitated and act as chemical stains stop the flow of the current, and whether or not the pure aluminium and the pure copper obey that law of disproportionate conductivity, is a question which I think no one can pronounce on, because I do not think any one has ever tried it under the required experimental conditions. Many observers have mixed aluminium and copper and determined the conductivity of the result, but that is a very different thing.

THE CHAIRMAN:—If it is a fact that aluminium bronze or alloys of aluminium and copper or other metals can be made which have a conductivity approximating the mean conductivity of their constituents, it would certainly be a valuable thing in electrical engineering, a thing greatly to be hoped for. I agree

with Dr. Waldo that the conditions under which Dr. Matthiessen made his tests in regard to chemical and electrical methods and instruments were so primitive that his results are by no means final.

MR. WESTON:—I have used aluminium quite extensively. I think Dr. Waldo is mistaken about its conductivity. I have found it to be 60 per cent that of copper. There is very little variation in the various samples I have met with, and that is about the average figure. Of course, I think what Dr. Waldo says about the action of small quantities of silicon or iron in aluminium when mixed with copper to form an alloy, may have a very marked influence upon the conductivity and working of the alloy. Silicon has a very marked influence on the working qualities of steel, and I think also on the conductivity. There is another matter there that probably might be worthy of some investigation, it appears to me. The copper of commerce is commonly assumed to be extremely pure, and within the past few years there has been a quality of copper called tempered copper put on the market, which analysis shows to be substantially the same as good Lake Superior copper. But when you examine it for conductivity, it has about 60 or 75 per cent. of the conductivity of good Lake Superior copper. Now it has occurred to me that the tempered copper, which is produced by a secret process, so far as I know, is probably a mixture of copper and hydride of copper, which was discovered by Wurtz some few years ago, and is nearly as brittle as glass. A very small percentage of that hydride of copper added to copper would have a very marked influence, I should think, on the physical and electrical properties of copper. An analyst, unless he made a very careful examination of the copper, would be likely to miss such a thing as a small quantity of hydrogen or hydride of copper, and hence the published analyses of this tempered copper of which we have heard a great deal, have not been made, probably, with sufficient care to detect a minute trace of hydrogen or hydride. That is a field probably well worth exploring to find whether the hydride of copper, which is very probably formed in making copper, does not come into play here, both in the working properties and in the resistance of the alloys or of the metal itself.

DR. WALDO:—I would like to say that my own best determination of the conductivity of the best aluminium I could get was 66.04 per cent. of soft copper, but I never have seen a piece of aluminium which was higher than 99.03 per cent. purity. I have had a number of samples submitted to me which by ordinary analysis have run as high as 99.75 per cent and 99.08 per cent. But a rigorous analysis has always reduced the amount of the purity of the aluminium. My personal judgment is that the conductivity of aluminium might run very close to that of copper itself, and there has been no evidence on record that I know of which would tend to disprove that, and a good deal of our private work in our laboratory would go to show it.

PROF. THOMSON:—As a rather curious and amusing instance of how things may come to one, I remember about nine years ago that the first specimen of what they call tempered copper reached me and the agent came around and attempted to introduce it to us as a metal which had about 130 per cent. of conductivity, and this was backed up by a certificate of absolute test made at a laboratory here in New York. I immediately thought this was strange and I suspected what was the matter. The Wheatstone bridge was handy, and I asked for a short piece of the wire, gauged it and it turned out to have the conductivity Mr. Weston stated; the explanation was that the two sides of the bridge had been read in the wrong way.

MR. OBERLIN SMITH:—I was glad to hear Prof. Waldo say that we might perhaps get as much conductivity out of aluminium as copper, bulk for bulk, because if we could, although we might not want the pure metal for line wire on account of its tensile weakness, yet it could be used for field and armature coils. But of course if it is a great deal worse conductor than copper, in proportion to its bulk, it would be a bad metal for coils, because we would get a less number of convolutions in a given space with the larger wire diameter necessary, and thus the average distance of the layers would be further out from the core. It is certainly to be hoped that we shall make it pure enough to get a high conductivity, so that it may compete with copper in this respect. But in respect to line wire, where we do not care so much what the bulk is, it is to be hoped that the alloys of aluminium which do show great strength, as for instance when adding from 6 to 10 per cent. of copper, will give a good conductivity—that we may indulge in longer spans, when desirable.

THE CHAIRMAN:—As we are on the subject of aluminium, and as one more paper on this interesting topic is on the programme —the last one—I think we should now take it up—the subject being "Shall Aluminum be Alium" by Mr. Oberlin Smith.

Mr. Smith read the following:

A paper read at the General Meeting of the American Institute of Electrical Engineers, New York, May 21st, 1891. Vice-President Crocker in the Chair.

SHALL "ALUMINUM" BE "ALIUM"?

BY OBERLIN SMITH.

Although having the honor of being a member of this distinguished society but a very short time, I take pleasure in accepting an invitation from your Committee on Papers and Meetings to say a few words regarding a proposed shorter and more euphonious name for the beautiful and valuable metal which must, perhaps, now be ranked as base, rather than precious, only because recent metallurgical and electrical science has made such wonderful strides in cheapening its production.

In February, 1890, I had the pleasure of reading before the American Institute of Mining Engineers, at their Washington meeting, a paper entitled "Aluminum in Search of a Nickname." In this essay reference is made to the fact that the nickname "Al" had already been irreverently introduced by many of the people engaged in manufacturing and selling it, in accordance, obviously, with the general tendency of our modern rapid life to condense all our commercial nomenclature into as small a compass as possible. In witness of this we, in America, as well as many of the denizens of other English speaking countries, have generally adopted the word "wire" as a substitute for "telegraph" and have, by successive stages, shortened the term "elevated-railroad" into "elevated-road," "the elevated," "el" and finally "L"—the latter being spelled as briefly as possible by the ingenious device of using only one letter.

In accordance with this labor-saving tendency in regard to our tongues and pens, it certainly seems desirable that a shorter name than "aluminium," or even "aluminum," should be found for a metal which is destined to be so largely used in the further evolution of our modern mechanical life—and probably not a little in

the electrical engineering of the future. In the paper before mentioned, reference is further made to the fact that the names of the commercial metals now in use, are nearly all short, crisp, words of one syllable, as gold, tin, zinc, lead, bronze, brass and steel, while the most common two-syllabled ones, nickel, silver and copper, are also simple and easily spoken.

In further arguing the point regarding aluminum, I suggested as possible future names "Aluum," "Alium," "Alim" and "Alm," as being suggestive of the original word for which they might become substitutes, and yet euphonious and easily pronounced. Since the essay in question was printed, my proposed reform has attracted considerable attention in the technical papers, the general preference appearing to be for the word "Alium" rather than for the others mentioned. This word has also been suggested independently by no less eminent a scientist than Prof. Elihu Thomson, who did not know that I had already proposed it. It has been heartily endorsed by Mr. Eugene H. Cowles, a well-known authority upon this metal, in an article in the *New York Tribune*, and by Mr. T. Commerford Martin, Past President of this society, in the columns of the *Electrical Engineer*.

Upon thinking over the matter further, I myself prefer this word to the shorter ones suggested, alim and alm. The latter, as Prof. Thomson suggests to me, reminds one of alms-giving, and I think the former looks and sounds too much like "alum." It is true that "Alium" has three syllables, but they glide so smoothly into one another that it is scarcely harder to pronounce than the shorter ones mentioned, while being much pleasanter to the ear; and, as Mr. Cowles states in his interesting article, agrees in its termination with many other words used by the chemists to signify metals—as, *e. g.*, sodium, calcium, etc. Very probably these same chemists would consider it appropriate to use the word "alia" for the oxide now called "alumina" I do not feel sure about the desirability of this, but cannot, at this speaking, see any objection to the word.

It is earnestly to be hoped, if we are ever to have a shorter name than either of the two present ones, that some decided step will soon be taken by the proper authorities, if their existence and identity can be determined, to establish, once for all, a shorter and more convenient name. If the leading technical journals in this country and abroad could be induced to agree upon the word "Alium," or something better, if such can be suggested,

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the electrical engineering of the future. In the paper before mentioned, reference is further made to the fact that the names of the commercial metals now in use, are nearly all short, crisp, words of one syllable, as gold, tin, zinc, lead, bronze, brass and steel, while the most common two-syllabled ones, nickel, silver and copper, are also simple and easily spoken.

In further arguing the point regarding aluminum, I suggested as possible future names "Aluum," "Alium," "Alim" and "Alm," as being suggestive of the original word for which they might become substitutes, and yet euphonious and easily pronounced. Since the essay in question was printed, my proposed reform has attracted considerable attention in the technical papers, the gen-al preference appearing to be for the word "Alium" rather than for the others mentioned. This word has also been suggested independently by no less eminent a scientist than Prof. Elihu Thomson, who did not know that I had already proposed it. It has been heartily endorsed by Mr. Eugene H. Cowles, a well-known authority upon this metal, in an article in the *New York Tribune*, and by Mr. T. Commerford Martin, Past President of this society, in the columns of the *Electrical Engineer*.

Upon thinking over the matter further, I myself prefer this word to the shorter ones suggested, alim and alm. The latter, as Prof. Thomson suggests to me, reminds one of alms-giving, and I think the former looks and sounds too much like "alum." It is true that "Alium" has three syllables, but they glide so smoothly into one another that it is scarcely harder to pronounce than the shorter ones mentioned, while being much pleasanter to the ear; and, as Mr. Cowles stat[illegible] in his interesting article, agrees in its termination with [illegible]her words used by the chemists to signify metals—as, [illegible]um, calcium, etc. Very probably these same [illegible]sider it appropriate to use the word "alia[illegible] [illegible]alled "alumina" I do not feel sure about [illegible]his, but cannot, at this speaking, see any [illegible]

[illegible] earnestly [illegible] are ever to have a shorter [illegible] either [illegible] some decided step [illegible] be tak[illegible] their existence [illegible]tity ca[illegible] r all, a shorter [illegible]re con[illegible] al journals in [illegible]untry [illegible] ree upon the [illegible]-Aliu[illegible] be suggested,

and if, furthermore, the national engineering societies should see fit to endorse the same, the work of reform would practically be accomplished, as it is not probable that those who are strictly scientists only, would hold out against a popular verdict of the kind mentioned.

Possibly a practical method of starting this reform would consist in one of the societies mentioned, appointing a committee to distribute an argument of the case among the editors of the various technical journals, here and abroad, asking for replies as to how many of them would be willing to adopt the new word, should a majority of the whole number agree. This committee could also send communications to the other national engineering and scientific societies, requesting action, or, at any rate, an expression of opinion, upon the same subject. If each society proved willing to appoint one or more members upon a joint committee of conference, a favorable report by such a committee, especially if backed by the technical journals, would undoubtedly command sufficient respect to lead to the adoption of the name in question by the societies themselves, at a later period. Possibly, however, it would be better not to take so much trouble about a single word, but to wait until a "batch" of technical words needing reform could be handled altogether.

If we consider the length of the names "aluminium" and "aluminum," in comparison with other elements, we find that the former is the only five-syllabled one extant. In a list of 64 elementary bodies we find only 18 of them to be words of four syllables, while 29 have three syllables, 11 two syllables, and five one syllable, the latter all happening to be ordinary commercial metals. The question naturally arises, can we afford to put a material which is probably destined to such extensive use as is this comparatively new and beautiful metal in the category of these rarely used and many-syllabled elements which, as has been seen, are in a small minority of the whole number? Can we moreover, afford to let it stand alone, in the whole list of elements, as the only one (excepting, perhaps, mercury, which foolishly has a sort of a synonym) possessing *two* distinct and interchangeable commercial names, as is now the case?

In enunciating the words in question, we find that although they are somewhat musical in sound, especially the longer one, they are not easy to speak rapidly, especially by a person not in the habit of pronouncing them frequently. In writing them,

the evil is worse, for in the case of "aluminium" one must, to be sure one is right, analyze and count some 28 nearly similar up and down strokes of the pen after writing "Al." Otherwise, there is apt to be a "u" or an "i" too many or too few; and even if they are right, some discretion is required in ascertaining where to dot the "i" and the "i." If the casual reader does not believe all this, he is only invited to try it as often as has the writer.

Perhaps the easiest way to solve this problem is to write the first two letters, [illustrating upon blackboard] thus, and then make a diagram of an alternating current, thus, counting meanwhile with the voice one, two, three, four, five, six,—well, so on up to twenty-eight, being careful to maintain synchronism between tongue and fingers, each cycle of the former agreeing with a single vibration in the diagram.

In regard to the interest of this very progressive society in the improvement and development not only of aluminium, but of its name, I think there can be no question; as it is a metal which is already used to some extent in the electrical arts, and is destined to far greater use as it becomes cheaper and as new inventions are made for applying it. Especially is this the case in view of the recent discoveries in France regarding certain alloys of aluminium with small percentages of copper, which show that we can double its tensile strength while increasing its weight only about six per cent. I have not heard how its electric conductivity is affected by such alloying, but if these stronger metals shall prove to be nearly as good conductors (and possibly they may be better) than is the pure metal, we at once have a bright vista opening before us for telegraph and telephone line wires, of great conductivity in proportion to their weight, combined with durability and beauty. There are doubtless numerous other applications of the metal and its alloys to be developed in the near future, especially in electric railroading, where strength combined with extreme lightness is destined I think to be secured by materials and methods, the use of which will, even in this generation, produce a speed and safety of which we, as yet, can scarcely dream. In this abolishment of the old "dead-weight" system—this destruction of the cars of Juggernaut, so to speak—in which the electrical engineer is to bear so brave a part, shall we know one of the most useful materials of the new age as aluminium, aluminum, or alium?

DISCUSSION.[1]

DR. VANDER WEYDE:—If we look at the list of elements, there are a great many elements of which the names are longer than aluminum. Aluminum is in perfect accordance with the derivation of alumina. Platinum is derived from the Spanish name of it—platina. The gentleman thinks that shortening the name of of our elements is desirable. Let him extend it then to others. Take potassium, that is longer Let him call it potium. Alumina has been introduced long ago. That is, the oxide of aluminum. Several substances are named in the same way. The termination "a" has been changed to "um." If you cut it down and say "alium," it will be contrary to the regular rule with respect to all the other metals.

THE SECRETARY:—This question is coming on the shoulders of either the Secretary or the Editing Committee very soon, and I would make a motion that the spelling of the name be referred to the Editing Committee for decision. I have already begun on "aluminium," because the author of the paper which preceded this insisted that that was right, and gave practical reasons for it. I am now approaching another set of papers where they insist on spelling it "aluminum" and in accordance with editorial practice, it is proper that we should spell the same word in the same way through the volume. Therefore, I think it incumbent on somebody to instruct me on this matter in the course of the next month.

DR. VANDER WEYDE:—Mr. Chairman, I should propose to leave it alone. It will settle itself

DR. WALDO:—I had the misfortune a little while ago to give a popular lecture on this thing, and sitting in front of me was a boy and his mamma. This boy went to the high school. Referring to the early history of the subject, and Sir Humphrey Davy's work, I used the expression, "aluminium," whereupon I saw the lady look at the boy and nudge him a little, and the boy looked disconcerted as if he had been set right and did not think he was wrong after all. Later in the discourse, I came down to the modern treatment of the subject, and I recollected the vote of our own Board of Directors, that this should be called aluminum. I called it aluminum, and I saw the boy smile and turn to his mother and nudge her.

I am afraid that history and practice are going to be against the Institute. Alium is a good word, but "aluminum" and "aluminium" have the dictionary, and the real construction of the language back of them. Both are given in the dictionary, and both are in good practice. But if I were the Secretary, I would spell it as I began. In England aluminium is used extensively and in the United States, owing to the action of the Cowles company and their circulars and our own, the expression aluminum

1 By Messrs. Vander Weyde, R. W. Pope, Waldo, Nichols, Lockwood, Alex. S. Brown, E. Thomson, Weston and Crocker.

has grown. I doubt whether at this stage, when the literature is as extensive as it is, it would be expedient to change the nomenclature of it. Certainly, if it were done, the movement might better originate with some society like the American Association for the Advancement of Science, which includes many chemists and physicists, and a number of our own members as well. I think a committee of the kind has been appointed by the American Association for just that question. Anyway it came up before a committee.

PROF. NICHOLS:—The committee has reported.

DR. VANDER WEYDE:—There is no precedent in the whole list of two i's close together. The United States government has adopted "aluminum."

THE SECRETARY:—The paper which you hold in your hand and from which you quote is the *Scientific American*. That is the way the editor spells the word. That does not prove that the government has adopted that form. I admit it is so spelled in the list contained in the paper, but my point is, that if the office style of the *Scientific American* requires that the word be spelled "aluminum" it would make no difference to the editor what form the United States government established.[1]

DR. VANDER WEYDE:—If it is in the pharmacopedia it is settled. It will come in the new United States pharmacopedia.

MR. LOCKWOOD:—Not more than ten years ago, I think, one of the sovereign states—at least one of the states that were sovereign before state rights were defined—decided for itself that its name thereafter should be pronounced Arkansaw. The *Springfield Republican*, commenting on that, said that any man who could spell Arkansas and pronounce it Arkansaw must be a jackaw. I will not presume to traverse that decision. I would call the attention of the Chair to the fact that subsequent to the change of pronunciation, one of the two senators from that state, insisted on his right to call the state Arkansas, and the other insisted on his right to call the state Arkansaw, and the president of the senate agreed with both gentlemen, and would call one senator the senator from Arkansas, and the other the senator from Arkansaw. I think Mr. Pope and the Committee, if there is one, could very profitably adopt the titles given by the authors of papers, and so please everybody. As regards the length of the name, it seems to me that it is not absolutely objectionable. If we remember that we are a nation, and a nation which does not care much for the length of words provided they are sufficiently sonorous; that when considering anything which is going to start, we speak of its being inaugurated; that if we send a message across the ocean instead of saying we wired it, as Mr. Oberlin Smith mentions, we speak of a *cablegram*. I will leave it to you Mr. Chairman, or any gentleman here who believes in pure

1. Since this discussion took place, the United States Census Office has issued a bulletin in which the word is spelled "aluminium."—[*Editor.*]

language whether cablegram is a decent word to have in a pure language—a word with no genealogy—the gram being taken from the end of tele, and cable put on the other end of it to show that it came through a cable a little longer than a river cable When we consider also that our profession is introducing such words as hysteresis, and reluctance and impedance, and reciptivity, I think we may, with very good reason, leave aluminium alone.

Turning now to another part of the subject, I notice that Mr. Oberlin Smith's paper speaks of "Al" being a nick-name. If that be so, I presume every element in the entire list has a nick-name, and why this element should not have a nick name as well as the others I fail to see. The fact that it is a symbol which might be considered a nick-name for Alfred or Albert Edward, or any other distinguished character, I do not think would interfere with the prerogatives of the element at all.

I come now to the up strokes and down strokes which glitter in multitudinous ranks on the blackboard. Now, anyone seeing the word as there written, would know what it was, whether the letters were properly formed or not. If it be an objection to have 28 strokes up and as many down, I suggest that hereafter the gentlemen simply write "al" and add an exponent.

Mr. Smith:—To what power?

Mr. Lockwood:—I do not care for the power. That is to be figured out subsequently. If we of the Institute should choose to do what we have no right to do, and say that so far as we are concerned, aluminium must cease to be, and aluminum must cease to be, but that alium may be, then I suggest that to prove ourselves consistent and to show the truth of the axiom that consistency is a jewel, we provide the names of other elements also with equal abbreviations. We might call selenium "sally," and molybdenum "molly." Vanadium would be a little more dignified, and we could call it "van." I know electricians whose names begin with that, and who would consider themselves honored by having a metal so named.

The gentleman objects to a name consisting of five syllables. He says it is the only one in the list which has five syllables. Mr. Chairman, I should like to know what element better deserves to have five sylables. If it is the only one, then from his own point of view he has so much the more to congratulate himself upon. But if there is any element in the entire list, entitled to pre-eminence in this respect, I should say it is this youngest born, that is going to be a perfect Benjamin, the son of our right hand. I think this question of the terminology of elements, is something for chemists to decide. Electricians have no right to choose names for themselves, or alter names already made. There are some names which are better when altered, but it does not seem to me that aluminium is one of them, and as I have said I think we can safely leave these things in the hands of

chemists. And, if we do, we may rest assured that persons who make names such as those of the ethyl group which no one can pronounce, will certainly look out for the conservation of aluminium. [Applause].

DR. GEYER:—We print on the cover of the TRANSACTIONS "The Institute as a body, is not responsible for the opinions expressed." I think in the same way the Secretary need not be held responsible for the spelling of the members. [Laughter.]

THE SECRETARY:—Mr. Chairman, I should hate to have anybody held responsible for the spelling of the author in all cases. I would like to have the TRANSACTIONS go out as an example of English according to some standard, whether it be a bad one or a good one. That point has certainly arisen, and as I say it is a matter of style, and we would vary from the practice pursued in all publications if we spelled the word one way in one number, and in another way in another number. That has happened in my experience. I think you will find disk spelled disc in one place, and in another place disk, due to the fact that the printer followed Worcester's dictionary and I used Webster's, and since we have come to an understanding on that point, we will endeavor to have uniformity. But the matter is one that I would like to have decided. I do not care who does it, whether it is the Editing Committee or myself.

DR. NICHOLS:—I merely arise to say that it is not many days since I received proofs of the report of the committee which has been referred to, a committee of the chemical section, I think, of the American Association, in which they give what they consider good reason for aluminium and recommend that that be the official name of the metal in this country. That gives some basis for action.

MR. ALEX. S. BROWN:—In the discussion last month Mr. Birdsall, I believe, asked if we ever called it alium, and I gave a reason for calling it aluminium. Probably some of the members now present were not at that meeting and have not heard the reason. In the manufacture of aluminium the oxide from which it is manufactured is alumina. Now, unless you are very careful in the pronunciation of alumina and aluminum, they sound very similar, and it is apt to be confusing; whereas if you call the oxide, alumina, which is the proper name for it, and the finished product aluminium, there is no bother at all. And then again, the name aluminium, I believe, was the original name that was given to the metal. In fact it was given to the metal before the metal was actually known to exist, as I mentioned in the paper I read at that meeting; and as that was the first name given to it, it seems to me it was entirely unnecessary to change the name.

PROF. THOMSON:—Perhaps, Mr. Chairman, we could settle this dispute by allowing everyone to call it what he chooses. We know that the word electricity is sometimes a rather troublesome

one from its length, and we find it, in the stations, often called the "juice." I think Mr. Lockwood would even recognize the word "phone" as a shortening of telephone—I do not mean authortatively—but he would understand it.

MR. LOCKWOOD:—I would know what it meant.

PROF. THOMSON:—Undoubtedly if there was a person whose name was John Smith and there was no other person around of the name of Smith we would be perfectly consistent in calling him Smith as we sometimes do.

MR. WESTON:—The great danger arises that in giving the metal a new name you will convey the impression to many minds that it is a new substance. It is not now an uncommon thing for persons to ask whether aluminum and aluminium are the same or different metals. If you give it still another name you will only increase the difficulty.

By giving the metal a new name you cannot stop the use of the older names—you cannot strike from the records of the past the older names. I think the only safe course to pursue in a matter of this kind is to follow the precedent established in such cases; namely, to stick to the name first given to the substance.

THE SECRETARY:—I might add that since our discussion of this subject before, where the practical question of the name came up in the manufacture of the metal, I have been informed by the editor of one technical journal that he had instructed his printers and proof readers that the word should be spelled "aluminium."

THE CHAIRMAN:—It seems to me that further discussion on this is unnecessary, because we are all familiar with the fact that many English words have different spellings and different pronunciations. Worcester has one pronunciation, and Webster another. A single authority, in fact, will give two or three different spellings and pronunciations, all of which are correct. We all suit ourselves under such circumstances, and I do not see that we can do better in this case. This Institute, in my opinion, has no right to dictate to other persons on this subject, because it does not concern us particularly.

MR. OBERLIN SMITH:—I think my friend down there who, apparently, must have a personal liking for nick-names of all sorts, misunderstood me if he thought that I mistook the chemical symbol for "Al." Of course we all know that it is written so in chemical nomenclature, and pronounced "A, L." But I am informed on good authority that at both the aluminium factories in this country, the workmen, clerks, etc., do nick-name it in this way, and have done for some time past. I do not think there is any question but that it would be better to have a shorter name. I am not crank enough, however, to think that this society or any other one, or two, or ten societies in the world can change a thing of this kind suddenly. I have no idea that there will be any reform immediately, but some have thought that a change

would be desirable, and are agitating it at this time. I am inclined to think that of the two old words "aluminium" is the more correct, though I have used the word "aluminum" because it is shorter. It certainly is open to the objection that it sounds like "alumina," when pronounced quickly. It has been said that one prominent engineering journal has adopted "aluminium." I will report the fact that another prominent engineering journal has adopted "aluminum." Long technical words which are used frequently in commercial life will no doubt be shortened in some fashion, by somebody. Many of them which are now being shortened accidentally will be shortened authoritatively. I am perfectly willing to let the words of my paper stand as a prophecy only. That some short word will come after a while I have full faith. Meanwhile, if we go on using the longer one of the two, the reform will come all the quicker on account of the nuisance of writing it.

I believe there is a motion not seconded. My friend Mr. Pope has been anxious to get some one to second it for an hour or two past. So I will second it—that the spelling of the word in the TRANSACTIONS, aluminium, or aluminum, be left to the Editing Committee.

THE CHAIRMAN:—As a matter of fact I presume it would be anyhow.

DR. WALDO:—I do not see by what right any publication committee could change the text of a scientific communication in this society or in any other. I am quite sure that if I sent a paper in and spelled the word aluminum, if it were changed by the committee or a typewriter or any one else, I should object. I think that would be the disposition of any one who used words advisedly.

THE SECRETARY:—It would be a little awkward in a discussion like this for instance where one gentleman speaking on the subject called it aluminum and the next man called it aluminium. It would be rather of a reflection on some one as to the editorial work.

MR. OBERLIN SMITH:—In seconding that motion I did not mean to say that a committee had any right to alter the spelling of an author, because that is a matter of quotation, and we ought to spell quotations in the author's way. I have had experience in this line, where the editor (I beg pardon, I mean printer) did not follow copy. In fact, my friend Dr. Wahl sent me the proof of a lecture, only the day before yesterday, where somebody had altered "aluminum" all the way through to "aluminium," and I had to change it all back again. I suppose it will be changed again when it comes to me in solid print. I assume that it would be this committee's duty to spell the word in the way the author spells it; but in the case of things said by the editors themselves, and in the reports of verbal discussions where they cannot very well pick up the exact spelling from the reporter's notes, the editors ought to have a standard, and *not two standards.*

THE CHAIRMAN:—I would suggest that the Editing Committee has jurisdiction over a matter of this sort without referring it to the Institute as a body, but the Chair will entertain that motion if it be the pleasure of the meeting. Mr. Pope, do you wish that motion to be considered?

THE SECRETARY:—It is immaterial to me as the Editing Committee can take it up. But as long as we were discussing the question I thought that if it were referred to the committee and settled, that that would end it so far as this meeting is concerned. If it had not been for this discussion, the matter would have gone to the Editing Committee in the natural course of events.

MR. OBERLIN SMITH:—As I understand it, the only point is whether the committee shall have a uniform standard for what they write themselves, or whether they shall do it both ways, or either, as they might do if nothing was here settled regarding uniformity.

THE CHAIRMAN:—Well, Mr. Pope, if you want to put that matter before the Institute please restate your motion.

THE SECRETARY:—My motion was that the question of the spelling of this word be referred to the Editing Committee for decision.

THE CHAIRMAN:—It is moved and seconded that the spelling of the word "aluminium" or "aluminum" be referred to the Editing Committee for their decision in regard to its use in the TRANSACTIONS.

[The motion was carried.]

THE CHAIRMAN:—There seems to be no further discussion on this paper, and I believe that the Secretary has several announcements to make.

THE SECRETARY:—Mr. Martin of the Entertainment Committee of the Electric Club, gives me this list of the articles on exhibition, forming the collection which has been referred to on our programme: [reading list.]

I have also received at a late hour and after our programme was made up, a paper by Mr. Sydney F. Walker, one of our foreign members, entitled "Notes on Electricity in Mining Work."

A paper read at the General Meeting of the American Institute of Electrical Engineers, New York, May 21st, 1891. Vice-President Crocker in the Chair.

NOTES ON ELECTRICITY IN MINING WORK.

BY SYDNEY F. WALKER, OF CARDIFF, ENGLAND.

When the writer was asked to contribute a paper to the TRANSACTIONS of the Institute, his feeling was that he had far more to learn from his confreres on the other side of the Atlantic than they could learn from him, and it is with considerable diffidence that he now ventures to forward the following notes upon a branch of electrical work in which he has had some experience.

Electricity has been used in mines in this country:

For signalling on engine planes and in shafts.

For speaking from point to point of the mine.

For shot firing.

For lighting; and for transmitting power to distant parts of the mine, to work pumps, coal cutters, etc.

It should be mentioned that electrical work in mines, so far as the writer's experience goes, is different from every other kind of work that the electrical engineer has to deal with. The apparatus is placed in the hands of men who are often altogether ignorant of electricity and the conditions under which it has to work are often many times more severe than any that would be met with elsewhere.

Damp is in a great many mines a constant feature, and so also are falls of roof, damage to roadways and many constantly recurring accidents, such as ropes breaking, horses taking charge, etc. Further, it has happened in this country that those who have designed apparatus for mines have never even seen one, and would therefore consider what experience has proved to be the ordinary requirements of successful working, simply ridiculous.

The signals that have been used are for ringing the cage up and down the shaft, and for ringing to the engine house and to various points on the road, where mechanical haulage is used.

The ability to stop a train of wagons instantly from any part of the engine plane has proved of such immense service that the engine plane signal has been adopted almost universally in the coal mines of the United Kingdom.

In this signal, as will be doubtless well known, connection is made between two naked parallel iron wires, which represent opposite ends of the circuit. As many as eight bells have been rung together on this plan, from any part of a haulage system extending over several miles.

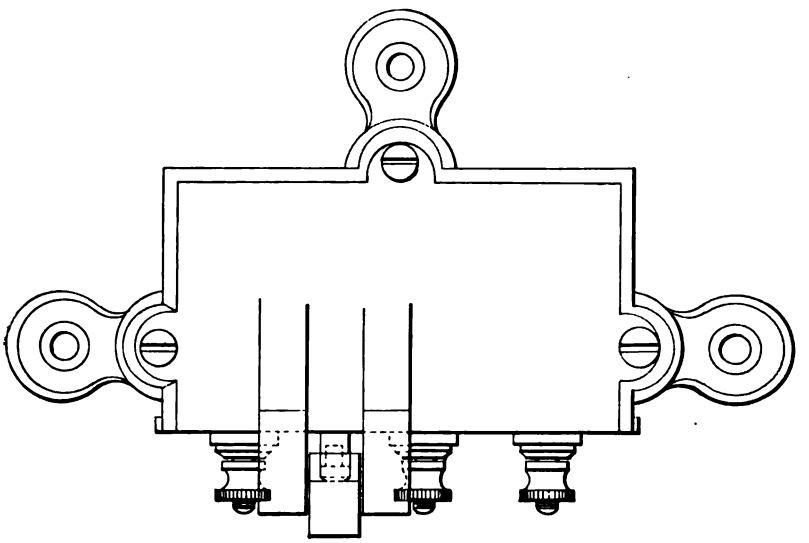


FIG. 1.

The great difficulties that have been met with in maintaining the engine plane signal are:—

The practical impossibility of keeping up the insulation, and the therefore constant drain upon the battery from the leakage current, etc.

The numerous joints that are made in the wires, owing to the constant breakage; each joint introducing considerable resistance into the circuit from its being not only unsoldered, but not even tightly twisted, or the ends of the wires clean.

The secret of success with this signal and with the shaft signal has been, making everything very strong, having a large reserve of power everywhere, and making the battery of large cells.

The electro-magnets, for instance, of the author's mining bells, are turned out of 2″ and 2½″ iron and with the battery power used with them would work through 50 miles of the wire employed—No. 8 S. G. Batteries of the largest sized Leclanche cells, if plenty of them be allowed, will stand well with these bells—the signal often going on working for twelve months without any attention beyond repairing the wires when they are broken.

The principal difficulties with shaft signals are:

The wet which is always present at the bottom of the shaft where the ringing key is placed, and the liability to damage of the insulated wire that is used for line and battery wires, more

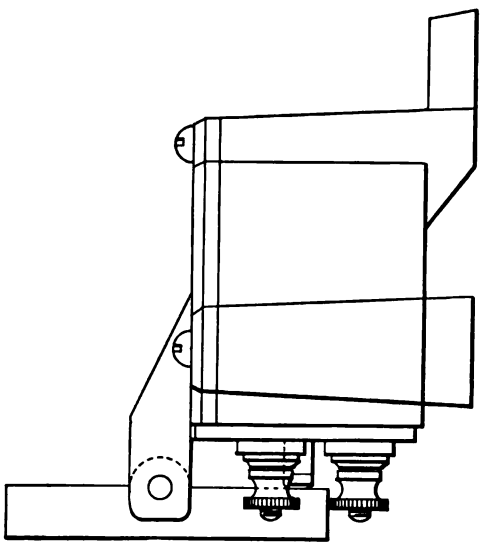


FIG. 1*a*.

particularly the latter. Falling coal damages the covering of the wire. admits moisture and the wire is soon parted.

Wet also plays great havoc with the connections to the ringing keys and with the contacts.

These difficulties have been overcome in the writer's practice, by fixing strong and well-covered wires in the shaft, protecting them with boarding, having no battery wire in the shaft, and by the design of the key shown in Figs. 1, 1*a* and 1*b*.

The key case is made of cast-iron and brass fitted together with flanges to exclude moisture as far as possible, so that the contact lever is unaffected by the ever-present damp.

An external lever of iron operates the internal contact lever by means of the sliding pin shown. The terminals of the key are outside the case, so that the wires can be cleaned off from time to time, and the signal kept going with little trouble. Where the terminals are hidden, no matter how carefully they are protected, damp always finds its way to the connection and destroys it, while the presence of the fault may be unsuspected.

The same remark as to batteries, that was made with reference to engine plane signals, applies here. The cells should be of the largest size and plenty of them, so that they may stand the drain

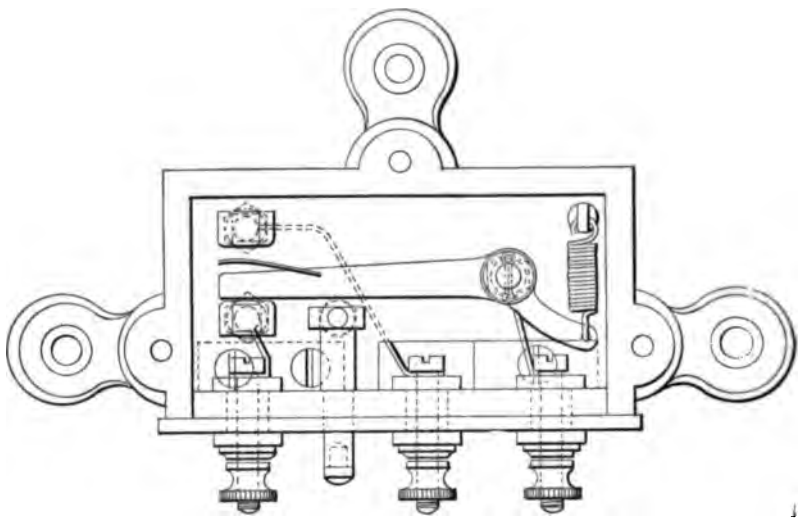


FIG. 1*b*.

of the leakage current often unavoidable, and of the incessant ringing, without requiring too frequent attention.

Signals are also used on the surface, for locomotive or haulage roads, but these can be dealt with in the same manner as ordinary railway signals.

Tin and lead mines have hardly adopted electricity at all in any form, partly on account of the conservative tendency of the owners, and partly on account of the enormous quantities of water present, and of the peculiar nature of the working of the mines themselves. The water present is usually strongly impregnated with metallic salts.

One tin mine in Cornwall tried a telephone between a level some 200 fathoms down and the surface, but the whole of the

working parts were dissolved away in no long time. One telephone, however, has survived in a Cornish mine, between a level 600 fathoms deep and the surface, at Dolcoath, near Camborne; and a great boon it has been. Probably this fact has kept it alive. Any one who has been down a few hundred fathoms, either by ladder or man engine, in a tin mine, will thoroughly appreciate this.

Telephones have been very little used in coal mines, owing to the monopoly which has prevailed. In fact, they have only been used for surface work, such as from colliery to shipping port and therefore need no remarks here. The monopoly being about to expire, we are hoping for big developments shortly.

For firing shots by electricity small hand magneto machines giving from 20 to 40 volts are used, and they provide current for firing fuses formed generally of two pieces of copper and bedded in gutta percha, their ends being covered with a detonating composition of high electrical resistance which is fired by the passage of the current through it.

The principal difficulties in connection with shot firing have been:

The loss of power of the magnets, due to loss of magnetism and the uncertainty as to the voltage required by individual fuses.

It would appear as if the fuses that are used in this country are made without any gauge so that it is no uncommon thing to find one or two of a batch of fuses fire with as low a voltage as two or three volts, while others require 30 and 40 volts and even more. This uncertainty in the voltage has led to considerable trouble at the mines which use this system of firing shots. A batch of fuses will be taken into a mine and perhaps none of them, or only one or two, will fire. The exploder is brought out and tested, perhaps on a low voltage fuse, which it fires. It is taken into the mine again, when it may fire some and not others, and so on.

In a few mines where the electric light is installed, fuses have been fired by branch currents taken from the lighting service. As the voltage used for lighting is now always 100 or 110 volts, this plan gets over both difficulties, but in the writer's opinion, and, he imagines, in that of most members of the Institute, the practice is a very dangerous one. In some collieries in South Wales, batteries have been entirely displaced for working both the shaft and haulage signals, branches from the lighting service

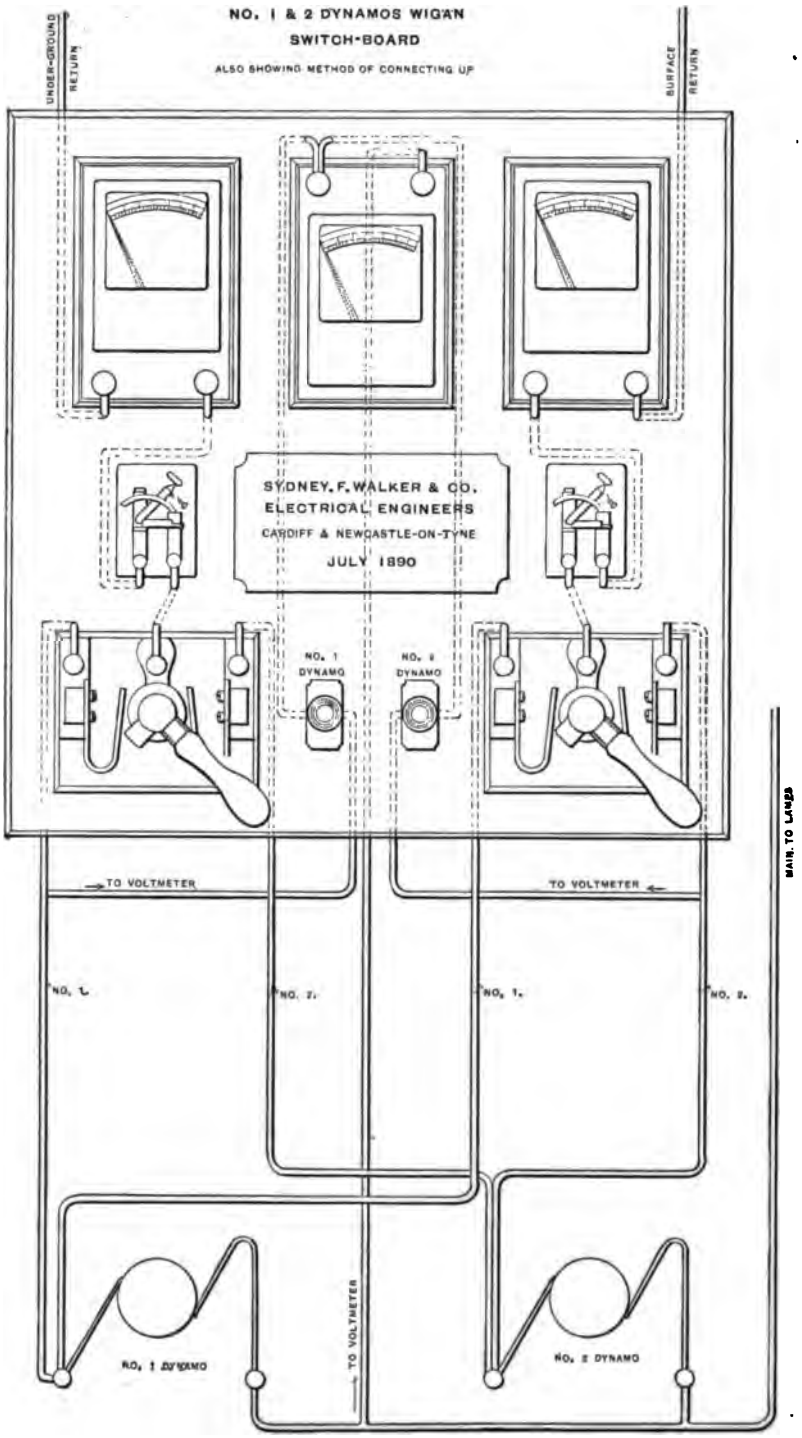
supplying the necessary current. An incandescent lamp is generally included in the circuit, so as to reduce the strength of the current passing through the bell coils. The lamp, which is arranged to light only when a signal is given, gives visual notice as well as the audible notice given by the bell. In one colliery, the current for firing shots is taken from the end of one of the iron signal wires. It has been found that only one connection is necessary, the other fuse wire being merely pushed into the hole that has been drilled in the coal for the charge, making contact, of course, with the body of the coal. This arrangement necessitates there being sufficient leakage on the lighting system to allow of the mass of the coal itself, with the rails, pulleys, ropes, etc., in connection with it, being used as a return.

In the instance mentioned, however, the arrangement has led to the somewhat startling result, that the polarity of the dynamo, placed on the surface, and at some distance from the pit's mouth, was twice reversed, apparently by electric currents induced in the bed of the coal itself. In another mine in Staffordshire, where old wire ropes are used for electric lighting mains, the leakage current is used to work the electric signal bells. In some of the South Wales collieries, where a branch of the electric light service is used to work the signals, it is no uncommon thing to see one of the iron signal wires sparking violently on the tops of a train of passing iron coal trams.

For lighting, the almost universal practice in mines is to have at least one compound wound dynamo, driven by a separate engine, laid down specially for the purpose.

In a few cases, the surplus power of the fan engine has been used, but the plan is not to be recommended, as it ties the electric light system too much to the requirements of the ventilating system. Added to this, it leaves no margin for the variations that occur even in the best made dynamos, between full load and light loads.

The plan adopted by the writer for a large colliery in Lancashire last summer seems the one to be preferred Two dynamos were provided, each having its own engine. The dynamos were constructed to give 200 lamps of 16 C. P. each, and the engines to work up to about 23 H. P. each. One dynamo was arranged to supply the surface lights and the other the underground lights, or motors if they were used; but a simple set of switches was provided, so that all the work, and each part of the work



NOTE, BOTH SWITCHES SHOWN OPEN ALL LAMPS OFF

FIG 2.

could be thrown on to either dynamo instantly, provided of course that the latter was running and furnishing its proper voltage. Fig. 2 shows the arrangement of the switch-board that was used in this case and Fig. 3, 3*a*, the two way switch. It will be noticed that the latter has good rubbing surfaces, and that the switch is completely locked on either side.

The engines in this case had only one cylinder each. They were fitted with very heavy fly wheels, and with Acme governors. The latter were arranged with adjustable springs allowing for a variation of 10 per cent. in the speed if required. This, however, was not found necessary, but as an instance of the variation heretofore referred to. The two dynamos were made from the

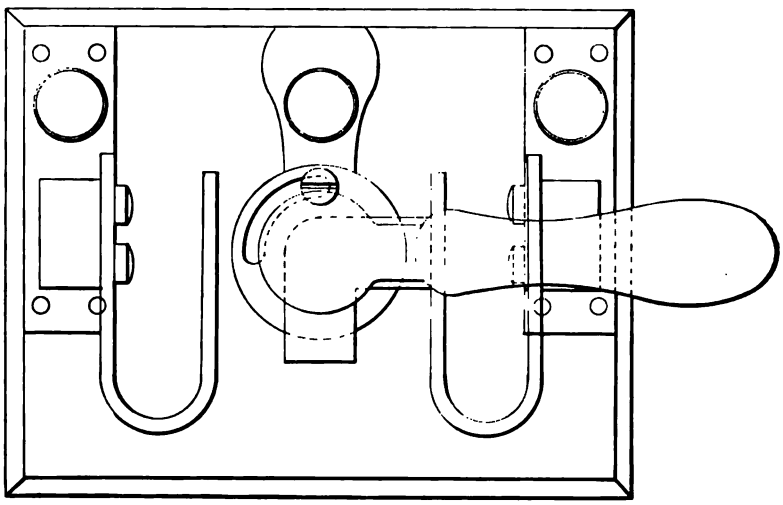


FIG. 3.

same iron, the same copper and by the same hands, yet one gave the specified voltage, 110 volts, at 785 revolutions, the other at 810 revolutions. The writer prefers two cylinder engines for electric lighting work, for the double reason that they work more steadily—badly spliced belts are less noticeable with them—and that the higher speed obtained with the smaller engine, and shorter stroke, enables countershafting to be dispensed with. It will be understood that the two dynamos were not connected electrically at all. Each did its own work, whatever it might be. In practice this resolved itself into one dynamo and engine running for 40 hours, and then the other, and so on. The colliery not being

fully developed did not require all the power provided. The reason the dynamos were not connected electrically was, the writer's experience has been that when this course is adopted, one machine takes more than its share of the work, so that its armature becomes strained if it is supposed to be working near its normal output. Though the two dynamos are supposed to be furnishing the same E. M. F. between their terminals, in practice they never do; and if one be only one-half volt ahead of the other, the latter not only cannot furnish any current, but its coils furnish a path for the current furnished by its more powerful partner.

Arc lighting has been almost entirely abandoned for colliery work, very largely, the writer believes, owing to his persistent

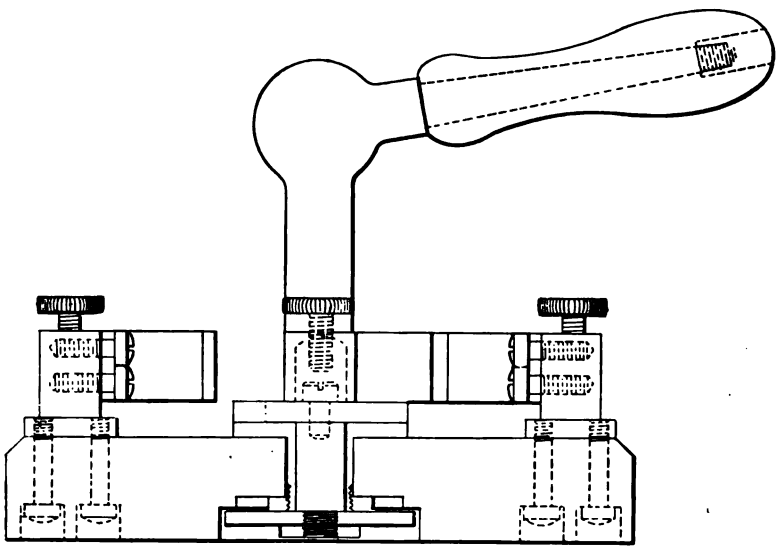


FIG. 3a.

effort in pointing out the solid advantages of large incandescent lamps. The improvement in the manufacture of 32 candle power, 50 candle power and 100 candle power lamps, and the advent of those of 200 candle power and upwards, has rendered electric lighting very simple; 100 candle power, 200 candle power or 300 candle power lamps are now placed over coaling screens and on pit banks, where arc lamps were formerly used, while in the yards and among sidings from 300 candle power to 600 candle power lamps are generally employed.

At the coal washing machine of the Dowlais Iron Company, where a powerful light is required for shunting trucks at night and on top of the coke ovens, 600 candle power and 1000 candle power lamps are used.

Some difficulty has been experienced with the 1000 candle power lamps in the open, owing, in the writer's opinion, to the manufacture of glass not having kept pace with the requirements of the lamp.

For underground engine-houses, offices, etc., the standard 16 candle power lamps are used, with an occasional 50 candle power or 100 candle power where a specially good light is required.

Callender's cables, with bitumen insulation, have been generally used for some years past for colliery work. Medium insulation being used for about the pit top and on the main roads underground where it is dry—heavy insulation where it is wet.

In the writer's opinion, this form of insulation is quite equal to the more expensive forms of rubber and gutta percha, if the wet be kept from it. For this purpose, for cables that have to be placed in the shaft, where it is invariably wet, the writer has the cable with heavy insulation overlaid with two thick layers of jute, put on the reverse way, and then with two coatings of tape soaked in waterproof material. This covering appears to stand all the water that is found in a pit shaft for a good many years. In the writer's experience, india rubber will not stand for long if the wet gets to it, nor will gutta percha unless it be very pure and very thick. Since cable makers have taken to adding other material to the G. P. they appear to have taken three parts out of the life of the material, where damp is present. Of the few installations of arc lamps still working at collieries, one may be mentioned which was fitted up by the writer's firm in December 1882. It consists of five of the mechanical commutating Brockie lamps, worked from an old type Gramme dynamo, furnishing 200 volts and 10 amperes at 1400 revolutions.

The lamp, as will doubtless be remembered, has no regulating system, except an electro-magnet actuating a grip clutch. The electro-magnet is energized by a shunt current, brought direct from the dynamo, and this current is broken for an instant every 15 seconds, by a revolving commutator, placed near the dynamo.

There are three shunt or feed wires for the five lamps, the revolving commutator breaking each in succession.

The dynamo in this case is driven by a double-cylinder vertical engine. made by Tangyes of Birmingham, and the whole apparatus

has not cost £10 for repairs in the eight years it has been running.

Old wire ropes, as already indicated, have been used rather extensively in two or three mines. In some cases the old rope has been used as a return, in others as both lead and return. Mr. Sopwith, the manager of Cannock Chase Colllery has led the way in this matter, and others have followed him. It appears that when he determined to light his colliery by electric light, he decided to do all the work himself, and proceeded to study the matter very carefully. A paper given by the writer to the North of England Mining Institute had the honor of being well thumbed amongst others. In the early days of his apparatus, however, Mr. Sopwith had trouble with some of his cables. Wet got to them and parted them. He therefore decided to try old wire ropes. These he has placed in wooden troughs in the shaft, and filled in the space round the rope with a mixture of coal dust and pitch. For distribution on the surface he has had brick troughs built, in which flat wire ropes are laid, bedded in the coal dust and pitch mixture. Underground, the writer believes, the old ropes are just laid in the coal dust. Mr. Sopwith states that since he adopted the above plan his troubles have ceased. Others who have followed him have not always been so fortunate. In one case where the writer was asked his views on the matter, he pointed out that there would be great danger of sparks, and that it would not be necessary that the two ropes should be in contact with each other, or both with some other conductor, for sparks to pass. One rope might be in contact with a conductor, which, though not touching the other rope, touched another conductor, and the final contact or break of contact, causing sparks to pass, might be in such a position that it would be difficult to trace the connection.

He also pointed out, amongst other things, that wherever the ropes or their supports encountered moisture, troublesome electro-chemical actions tending to destroy the rope would probably ensue.

Both of these predictions were verified almost to the letter. Sparks were generated at the bottom of the pit from just such a series of connections as he had foreshadowed. One rope touched a sheet of galvanized iron at the landing stage at the pit bottom; that is to say, sometimes it touched and sometimes it did not. The iron sheet was bearing against an iron pipe which went a

little way into the workings and this or some other conductor in connection with it was bearing against one of the insulated conductors leading to the lamps and had rubbed its insulation through. The result was that sometimes the series of connections were complete and then broken, giving rise to sparks. In the same pit, one of the ropes before mentioned was eaten right in two at one of the landing stages; and in another pit in which the precaution was taken to fix one rope in the down cast and the other in the up cast, the iron supports of one rope, where they were driven into the pit props, were eaten into just as if they had formed part of a galvanic battery, or had been used as anodes in a plating bath. In another case where a naked iron rope was used as a return, the writer heard of a man receiving a very severe shock, while only 160 volts were in use on the system. It was an electric pumping plant.

The writer's feeling is therefore, that no naked conductors for electric lighting or power should on any account be used in any part of the mine and that the insulation should at all times be maintained as high as it possibly can be. Even for cables used to distribute current about the surface, the writer's invariable practice is to use insulated wire, for fear that the wires may be brought accidentally into contact and the attendant troubles ensue. The writer understands that in one mine in America, power was transmitted at a voltage of 280 or 300 volts, the rails being used as a return and lamps being worked from the same system, arranged two in series. If such an arrangement were used in this country, he would expect trouble to follow before long.

Not a great deal has yet been done in transmitting power by electricity for use in mines. There are several isolated pumping plants at work, one or two haulage plants, and one or two coal cutters.

The voltage used for pumping has been 300 and 500 volts, in one case as much as 700 being used. The plan that has been almost universally adopted for electric pumping plants has been to use a series wound motor, a shunt wound generator; both of sufficient power for the largest amount of work they may be called upon to do; and a separate engine whose speed can be varied in accordance with the requirements of the work. The most frequent application of electrical pumping plants has been for raising the water out of dip workings, which are often a long distance

from the shaft, either to the sump at the pit bottom whence it would be raised by the main pumping engine, or to the surface. In dry seasons this water may not be much, so that the pump is lightly loaded. In wet seasons the reverse would be the case. Up to the present these requirements have been met by an increase or decrease of speed at the engine, giving rise to higher or lower voltage at the terminals of the generator and motor, with greater or smaller driving power. The writer's view is that as the use of electricity for transmitting power becomes more general, this system will give way to the simpler one, having only two or three dynamos as generators, all running at the voltage of the lamps; the motors for the pumps, hauling engines, etc., being connected in parallel, just as so many lamps, and the variation of the loads on the motors being dealt with as so many lamps turned in or out would be. The regulation of the increased or decreased load on the pump with varying seasons, can easily be dealt with mechanically and without any waste of electrical power.

The largest return obtained from an electrical pumping plant in this country has been 40 per cent. in actual work done in raising water, of the brake horse power delivered to the generator. It is obvious, however, that the efficiency of the pump and the size of the delivery pipes must have a material effect upon this figure.

In the majority of the pumping plants that have been laid down in this country, the pump has been geared up with one or two wheels and then driven by a belt from the shaft of the motor. Quite recently, however, Messrs. Golden & Co., of London, have worked out self-contained pumping, hauling and coal cutting plants; the motor, gearing and machine being all on one bed plate. This, it will be seen, places electrical mining plants more on a footing with steam and hydraulic plants, as it is only necessary to place the combined machine on rails, run it into its place and connect the branch cables. In working this out, however, a somewhat unexpected difficulty arose. It is one, the writer believes, American engineers dealt with some time since in their tram-car work, and it is one which it would have been thought that skilled mechanical engineers would have foreseen. When a belt was no longer interposed between the motor shaft and the pump, so that the former had to start against a dead load without any flexible coupling between that would give—few armatures could be got to stand the strain; they broke up.

The reason of this was, of course, the flimsy nature of the

mechanical joint between the armature spindle and its core, which for electrical reasons is usually provided. A sound mechanical joint, properly distributed, got over the difficulty at at once.

Messrs. Golden have also provided a spark protector in the shape of a cover over the commutator, so that if gas is present where the motor is working it cannot get to the spark at the commutator.

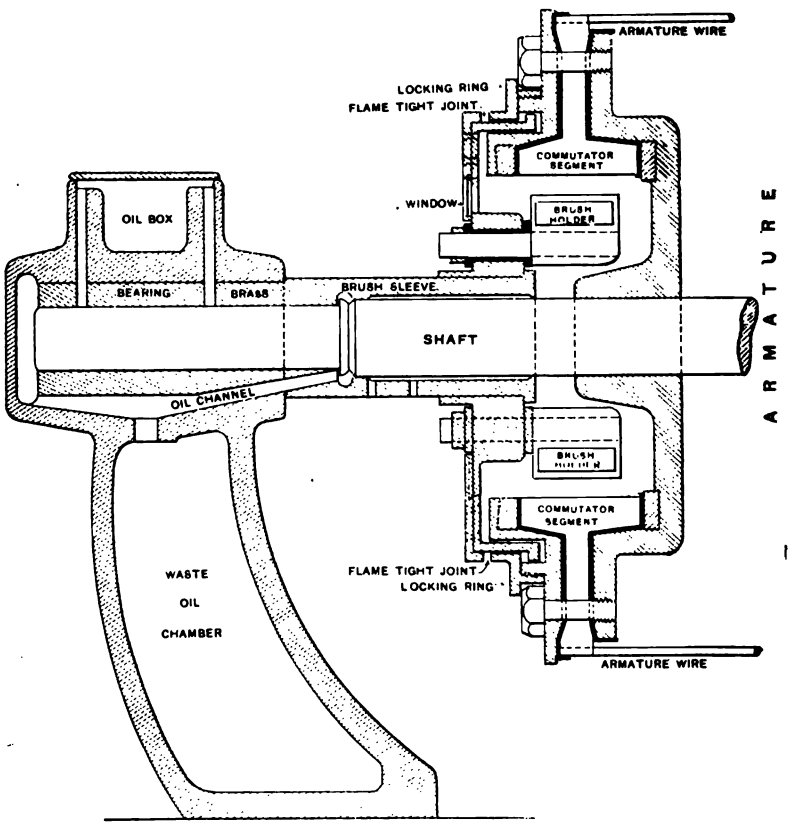


FIG. 4. Stokes's Safety Commutator.

Though this is, perhaps, a necessary concession to the feelings of mine owners; in the writer's opinion, it is both useless and unnecessary. His view is, as he expressed it at a recent meeting of the Institution of Civil Engineers, that you cannot bottle up the sparks, but you should reduce their tension below firing point.

Mr. A. H. Stokes, H. M. Inspector of Mines for Derbyshire, has attacked the problem in another way. He makes the commutator hollow and places the brushes inside. Experience will

prove, of course, if this is successful, but the writer's view is that it will be found to give rise to more trouble with the brushes than it gives protection from explosions. It will never be possible to see if the brushes are properly set, and any sparking which takes place within the very confined space available will cause very serious trouble from the heat developed (see Fig. 4).

In electrical haulage, the principal work has been done in Yorkshire: 40 per cent. in weight of coal and trains drawn up an incline has been obtained in one case, of the brake horse power delivered to the generator.

The principal difficulties in connection with haulage appear to be the matters of variation of speed and reversal of direction. Where the system consists of an endless rope always travelling in one direction, the matter is, of course, much simplified; but for variations of speed the use of resistances in mines is a matter that requires great care, simple though it seems to trained electrical engineers.

For reversals the writer's view is that the motor should always travel in one direction and the operation of reversing be performed mechanically. If the variation of speed can also be conveniently accomplished by mechanical means, it will be easier and safer. In the matter of gearing for reducing the speed between motor and driving drum or pump, practice has settled down after many trials in other fields, to what American experience has proved to be the best, viz.—spur gearing. A short time since, when the writer was studying this question, he sent to every institute in the United Kingdom for papers on the subject, but none could help him out. The universal practice with other appliances where reduction of speed was necessary was to use spur gearing. Why was this? Why not worm gearing? At last he wrote to the Secretary of the American Society of Mechanical Engineers, who kindly sent him a copy of a paper which had been read before his society, which gave the solution of the whole problem. From a careful series of experiments made by Messrs. Sellers, it appears that while you can get 98 per cent. return of your power with spur gearing, and rarely less than 90 per cent. without much wear; with worm gearing 75 per cent. was the utmost you could get by any means, 60 per cent. and less was more nearly the rule, and the wear was very heavy. A namesake of the writer's, Mr. J. Blake Walker, the manager of one of the Yorkshire collieries has introduced a system of electric haulage, in which

the old wire rope is used as a conductor, the motor travelling over it, and taking its current from it. The object of this plan is stated to be the saving of the power expended in driving the rope; but it appears to be forgotten that power is also expended in driving an electric current *through* a rope, just as in driving the rope round a pulley. The writer has not heard how far the plan has been successful, or if there be any margin between the above two figures.

The only other matter the writer proposes to deal with is that of the voltage that should be used in mines. His view is that while those engaged in a mine are becoming accustomed to the use of electricity, the voltage should be kept as low as possible; and he believes that though low voltage means a larger expenditure for copper, the interest on the difference between the cost of cables under say 100 volts and 500 volts, will be more than balanced by the saving in the cost of maintenance, owing to the great difficulty of maintaining the insulation with the higher voltage. Added to which, it will be a great convenience to work all the electrical apparatus from one source of supply. As already indicated however his views on this point, though accepted by mining engineers have not been by electrical engineers. The writer regrets that he will not be able to attend the reading of his paper in person; but will be pleased to reply to any criticisms or to answer any questions that may be asked; and may perhaps at some future day hope to have the opportunity of meeting his American fellow members.

Discussion.

[COMMUNICATED BY HOLLON C. SPAULDING.]

I have read Mr. Walker's paper with great interest, and I must add a little surprise, as showing a transition state which I had imagined the British electric mining engineers to have suffered from less than their American cousins. Certainly some of the make-shift methods described by Mr. Walker are not of the ultra-conservative ways which have been held up to us on this side of the water as types of English practice in some lines of scientific work.

While agreeing in general with the statements contained in the article, I may call attention at random to some points of difference in opinion and practice. In regard to the reversal of motors for instance, the writer's experience would certainly warrant the reversal of the armature itself by electrical means instead of more

cumbersome mechanical methods. From the smallest size up to electric hoisting machines of 60 horse power brake capacity, for example, no trouble has been experienced with this method, and we certainly see no reason for turning to earlier devices for accomplishing the same results.

While the question of spur vs. worm gearing is still an open one, the advantages of compactness and silence go a good ways in recommending worm gearing in some lines of work, and it is certainly a debatable point, whether the difference in efficiency amounts to more than a negligible quantity when a well lubricated worm and gear takes the place of the two pairs of spur gears. One example in particular occurs to the writer as an illustration. A 10 horse power motor having been in use for several months in a travelling crane, the worm having a maximum speed of 1600 revolutions per minute and being placed *over* the gear, both being entirely encased in an oil tight jacket and the teeth of the gear acting as paddles, delivering a constant supply of oil to the contact point.

I cannot quite understand Mr. Walker's difficulty in operating two or more generators in multiple, both machines supplying current to the same system of feeders, from which may be operated lights or motors for various kinds of work. What is known to our engineers as an "equalized bar," supplying a close connection between the armatures of various machines working together, seems to answer every requirement in the writer's experience.

The switch-board shown, while certainly ingenious in its arrangement, would apparently not answer our American requirements as the switches are all single pole instead of double.

As to the voltage to be used in mining operations, the American practice of nothing higher than 220 volts below the surface seems to correspond most satisfactorily with Mr. Walker's ideas.

The development of a line of air and gas-tight motors and accessory apparatus is certainly most interesting as increasing the field of usefulness and safety in fiery mines, but, without going into the subject in detail, is not the true solution of the problem of electric installations in fiery mines, the provision of such a thorough ventilating system as is offered to better advantage than in any other way by the flexibility and ease of manipulation of a low voltage constant potential power and lighting system?

[COMMUNICATED BY H. WARD LEONARD.]

Almost every English electrical engineer, who has visited in the United States, has been reported as being very much surprised at the inferior quality of our construction work here, as compared with that done in England. It is, therefore, with no small amount of surprise that I observe the methods which Mr. Walker describes as being in use in the mines in England The character of the work which he describes as having been done in connection with signalling system in mines in England is such that it is not at all surprising that he states that only "one telephone has survived" which has been installed for mining use, and that this one has survived is surprising, in view of the character of the work which he mentions.

The following is a quotation from his paper:

"The great difficulties that have been met with in maintaining the engine plane signal are: the practical impossibilities of keeping up the insulation, and the therefore constant drain upon the battery from the leakage current, etc.

The numerous joints that are made in the wires, owing to the constant breakage; each joint introducing considerable resistance into the circuit from its being not only unsoldered, but not even tightly twisted or the ends of the wires clean."

This statement indicates a lack of knowledge as to the necessary conditions in order to secure success and a lack of proper materials for the work undertaken which it is difficult to conceive. The quality of the mine fuses used in the mines in England appears to be inexcusably bad, judging by his statement that these fuses vary so greatly in the quality as to require sometimes two or three volts, while others require 30 or 40 volts at the generator in order to supply sufficient current to fire them. It is not surprising that this uncertainty has, as he states, led to considerable trouble at the mine which uses this system of firing shots.

Another quotation is as follows:

"In one colliery the current for firing shots is taken from the end of one of the iron signal wires. It has been found that only one connection is necessary, the other fuse wire being merely pushed into the hole that has been drilled in the coa for the charge, making contact, of course, with the body of the coal. This arrangement necessitates there being sufficient leakage on the lighting system to allow of the mass of the coal itself, with the rails, pulleys, ropes, etc., in connection with it, being used as a return."

It is difficult to believe that such an instance as this can be cited for any other purpose than to show how extremely unsatisfactory the use of electricity will prove when installations are made in total disregard of proper requirements and methods. That any one having any knowledge of the subject should voluntarily use a leakage current from a lighting system for the purpose of operating such important work as shafts and haulage

signals and the explosion of mine fuses seems incredible to any one who has had any connection with the use of electricity for important operations.

Mr. Walker mentions what he very properly calls "a startling result," namely that "the polarity of the dynamo placed on the surface was twice reversed, apparently by the electric currents induced in the bed of the coal itself." It is probable that the dynamos for incandescent lighting are shunt-wound, as is common practice in this country; and how any possible current produced by the dynamo, of the nature which he described, could reverse its field it is difficult to understand; in fact, appears ridiculous, especially in view of his statement that it was apparently by electric currents which were "induced" in the bed of the coal by a continuous current.

It is rather surprising that one of the reasons advanced by Mr. Walker why two-cylinder engines are preferable for electric tric lighting work is that "badly spliced belts are less noticeable with them." Belts which are so badly spliced as to occasion flicker in the lights are a feature of electric lighting work which I have not seen in the past eight years in this country where the plant has been installed by any one who has ever had any previous experience with the subject.

Mr. Walker states that he found it impossible, in his experience, to properly operate two dynamos in multiple arc with each other. He states that, "while the dynamos are supposed to furnish the same E. M. F. between their terminals, in practice they never do." Apparently Mr. Walker is totally unfamiliar with the methods which are in use throughout the world for equalizing the E. M. F. and consequent current of dynamos in multiple arc. It would probably surprise Mr. Walker greatly to see what is common practice with the Edison company in this country, viz: the operation of as many as twenty different dynamos, frequently of different sizes, all operating in multiple arc and dividing the current exactly in proportion to their sizes.

If we can judge from Mr. Walker's paper, it is common practice in the mines in England to use the old wire rope for electric conductors. That any such conductors should be used in a mine is only less surprising than that a party, whom he states studied the matter very carefully, proceeded to insulate the old wire rope by "a mixture of coal dust and pitch." Mr. Walker states that the gentleman who originated this novel insulation met with entire success; but I am not surprised at the further statement that "others who have followed him have not always been so fortunate."

In view of the character of the work which Mr. Walker describes, it is very amusing to notice his statement, that he understands that "in one mine in America the power was transmitted in a voltage of 280 or 300 volts, the rails being used as a return, and lamps being worked from the same system, arranged two in series;" and that "if such an arrangement were used in England he would expect trouble to follow before long."

Mr. Walker states that a short time ago, while he was studying the proper kind of gearing for haulage he "sent to every institute in the United Kingdom for information on the subject, but none could help him out." He seemed surprised to learn that, in other appliances, where reduction of speed was necessary "spur gearing was generally preferred," and he asks, "Why was this? Why not worm gearing?" And it is rather pleasing to learn that he at length secured "the solution of the whole problem" by receiving a copy of a paper which had been read before the American Society of Mechanical Engineers, which apparently had supplied to the English public, for the first time, data as to the comparative efficiency of spur gearing and worm gearing.

It seems hardly possible that Mr. Walker, in his paper, has described practice in electrical engineering which is common in England; and yet one cannot avoid the inference that he himself believes so. In view of the publications in the American journals—some of which have been copied in the English journals—regarding the character of mine installations in America, it seems entirely unnecessary to make any comparison between the kind of work which is done here, and the kind of work which is described by Mr. Walker. I can only say that no one of the difficulties which Mr. Walker has pointed out as the principal ones to be met with in mining has been considered as such by the Edison company, and that the actual important difficulties which are met with in replacing other power by electric power in mining installations have apparently been overlooked entirely by Mr. Walker. The transmission of a large amount of power over several miles and its utilization at the point of use for the purpose of operating concentrators, arc lights, incandescent lights, hoists, pumps, locomotives and mining drills—both rotary and percussion—involves problems in electrical engineering beside which the difficulties which Mr. Walker has touched upon fall into utter insignificance; and yet it is work of this character which is going on daily in this country by more than one of the leading companies.

A paper read at the 59th Meeting of the American Institute of Electrical Engineers, New York, September 22d, 1891. Vice-President Lockwood in the Chair.

ON THE RELATION OF THE AIR GAP AND THE SHAPE OF THE POLES TO THE PERFORMANCE OF DYNAMO-ELECTRIC MACHINERY.

BY HARRIS J. RYAN.

The object of this paper is not to deal with the subject in a new light, but to add to its literature a limited amount of data, the deductions from which go to establish the correctness of the ideas, and the utility of the suggestions put forth in the papers read by Messrs. Swinburne and Esson, at the meetings of the Institution of Electrical Engineers on Feb. 13 and 20, 1890. Up to the time of the publication of these papers, the air gap was usually treated by contributors to electrical literature as an evil in a dynamo, having a necessary existence, and the smaller that it could conveniently be made the better. The shape of the poles had often been spoken of as having a somewhat decided effect on the performance of the dynamo, while but little had been said regarding the cause of such an effect.

There exists some difference of opinion as to what should be known as the number of ampere turns on an armature. For our present purpose we will assume that the

$$\text{Armature ampere turns} = \frac{\left\{\begin{array}{l}\text{The No. of conduct-}\\ \text{ors on the surface of}\\ \text{the armature}\end{array}\right\} \times \left\{\begin{array}{l}\text{Strength of cur-}\\ \text{rent in the arma-}\\ \text{ture conductors}\end{array}\right\}}{\text{Number of poles.}}$$

Referring to Fig. 1, it is evident that when we consider the magnetic forces acting in a working dynamo by the route O, I, J, O, that the entire number of ampere turns on the armature are directly opposed in action to the ampere turns on the field. By the route O, M, N, O, all the ampere turns on the armature except those that lie between the double angle of lead G, E, are acting

with the field ampere turns, while those between G, E, are opposed to the same. Therefore by this route, the total number of ampere turns actually aiding the field ampere turns is the total number of armature ampere turns, minus twice the number of ampere turns that lie between the double angle of lead. By the route O, K, L, O, the number of ampere turns acting is the number of ampere turns on the field, minus the number of ampere turns that lie between the double angle of lead. We can then estimate with ample practical accuracy the magnetic density in the air gap at all points, for any given total amount of magnetization through the armature. The

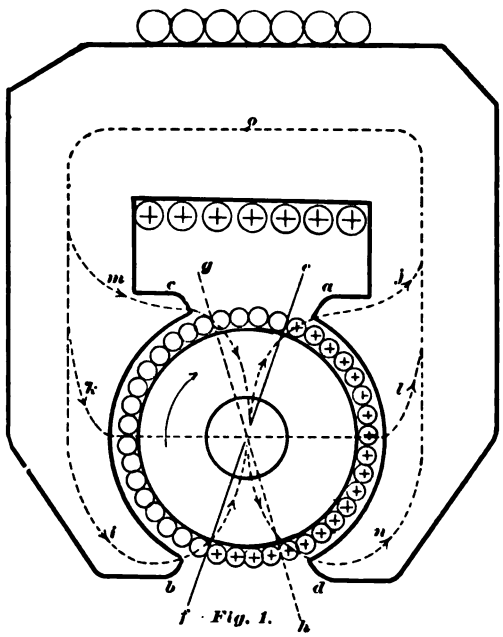


Fig. 1.

ampere turns that lie between the double angle of lead are opposed to the action of the field ampere turns at all points. It is evident that the portion of the armature ampere turns not included between the double angle of lead will increase the magnetization through the air gap by the route O, M, N, O, just as much as they diminish it along the route O, I, J, O, as long as magnetic saturation does not take place in the strengthened pole corners C, D. If the pole corners are thin, as in the types shown in figures 2, 3 and 5, saturation is apt to occur. It is then that the magnetic resistance increases by the route O, M, N, O, and the magnetic

density by this route is no longer increased by the same amount that it is diminished along the route O, I, J, O. On the other hand, when the pole corners are fashioned as seen in figures 4 and 6, so that saturation in the strengthened pole corners can not occur in practice, the current in the armature can produce no modification of the total amount of magnetization through it, other than that which is produced by the action of the ampere turns that lie be tween the double angle of lead. This action can always be com pensated for by putting an equivalent number of series ampere turns on the field acting with the field ampere turns. The double angle of lead can be determined with sufficient accuracy, for with pole corners slightly extended at the centre, see Fig. 11, the diameter of commutation at all loads is very near the weakened

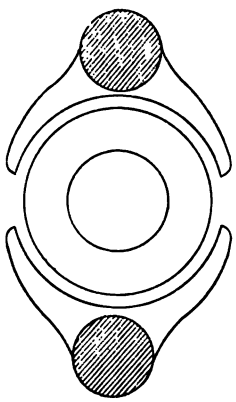


Fig. 9.

pole corners. The pole corners are slightly extended at the centre, so that the coils always enter the field of the weakened pole corners gradually. The E. M. F. developed in the coils as they pass under the poles can never be far different from that actually needed to reverse the current in the coil when passing under the brush. In this way the point of commutation in a dynamo can be kept the same when carbon brushes are used without undue sparking, as long as the armature does not reverse the magnetization under the weakened pole corners.

From the discussion of the magnetic relations of an armature to its field in a dynamo in connection with Fig. 1, it is seen that the magnetization in the air gap under the weakened pole corners becomes zero, when the armature ampere turns are equal to the

ampere turns on the field, whose magnetizing force is impressed between its pole faces through the armature. This impressed magnetizing force is that due to the difference between the total number of ampere turns on the field and the number of ampere turns required to set up the magnetization through the field cores, from pole face to pole face. In order to commutate the current without spark at the commutator, the magnetization in the air gap under the weakened pole corners dare never be allowed to become zero. It follows then that the field ampere turns impressing a magnetizing force between the pole faces must always be somewhat in excess of the maximum number of ampere turns on the armature. The amount of this excess need only be sufficient to insure a positive field at A and B, Fig. 1, strong enough to reverse the current in the coils as they are commutated. When a certain amount of magnetization is to be set up through an armature with the application of the magnetizing force of a given number

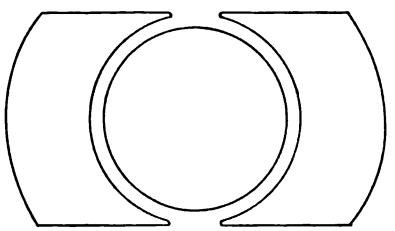


Fig. 3.

of ampere turns impressed between the pole faces, we must provide the requisite amount of magnetic resistance between these pole faces. The value of this resistance will have to be such that the impressed field magnetizing force will establish the desired amount of magnetization. This resistance in most cases is best provided for in a proper length of air gap. In general it is found best to avoid heating in the armature core as far as consistent, by the use of comparatively low magnetic densities for wrought-iron. The magnetic resistance of the armature core under these circumstances is very small and may be neglected.

The magnetic resistance between the pole faces is occasionally provided for largely, either through a saturated core of a ring armature, saturated lugs on armatures where the wires are placed in grooves, or both. This in addition to what air gap may be necessary from a mechanical point of view, go to make up the total amount of magnetic resistance that is provided between the

pole faces. Machines of this order have been developed largely through the old and rather expensive method of experimentation. This method has given us some types in which ordinary results are arrived at, through rather extraordinary means. Take the case of a machine with a ring armature, wires wound in grooves, a very small air gap, and poles shaped somewhat as shown in Fig. 2. Such a machine operated as a dynamo, may require only a quarter of the number of ampere turns that it will have on the armature at full load for field excitation, in order to produce a certain E. M. F. at a given speed. Yet this machine produces a fairly constant potential at the brushes under all variation of load, and without undue sparking at the commutator in the following manner: For the production of a constant E. M. F. at constant speed, the total magnetization through the armature must remain constant. At no load one fourth of the ampere turns needed on

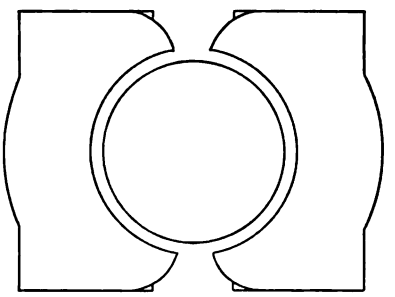


Fig. 4.

the field at full load, are provided by a shunt winding. This shunt winding is sufficient to set up the total amount of magnetization for the production of the normal E. M. F. of the machine when there is no current in the armature. Now in order to take the normal current from the armature without reversing the magnetization under the weakened pole corners, three times as many series ampere turns as there are shunt ampere turns must be added to the field. The addition of these series ampere turns must not increase the total amount of magnetization through the armature, which is accomplished by the thin pole corners. The strong pole corners become saturated when the armature is furnishing even a small amount of the normal current for which it is designed. For most values of the current then, the armature ampere turns, tend to diminish the magnetization under the weakened pole corners, but can not increase it correspondingly

under the saturated pole corners. The action of the series ampere turns on the field prevents the reduction of the magnetization under the weakened pole corners to zero, while the saturated portion of the pole pieces prevents the increase of the total magnetization through the armature, and thus a constant potential is maintained.

The "armature characteristic" IV. plotted in Fig. 7, was taken from the machine of the above sort, built with cast-iron

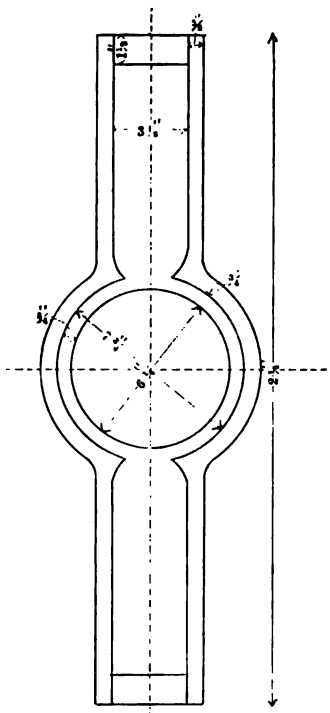


Fig. 5.

fields. The cross-section of the field cores proved on trial to be too small, and became strongly saturated at full load,—while they were quite a little under the point of saturation at no load. This curious result for a constant potential generator was due to the increased magnetic leakage, produced as the series ampere turns on the field came up with the load. Saturation took place as the curve indicates, when the armature furnished a current of about 140 amperes, and no possible compounding could ever make this

generator produce even approximately a constant potential, with variation of load. Steel cores of the same dimensions were substituted for the cast-iron cores. Saturation did not occur in them due to magnetic leakage. The pole corners were very thin, as in Fig. 2, and the "armature characteristic" III. was obtained. The machine was then furnished with a shunt winding that produced a slightly smaller number of initial ampere turns than curve III. indicates as required to produce 125 volts, and with series turns at such a number that the total number of ampere turns on the field for any current developed by the armature is shown by the broken line drawn through curve III. It was under these conditions that the machine performed in the manner described above, and did not vary more than 5 per cent. from the normal E. M. F. on either side, or a total variation of 10 per cent.

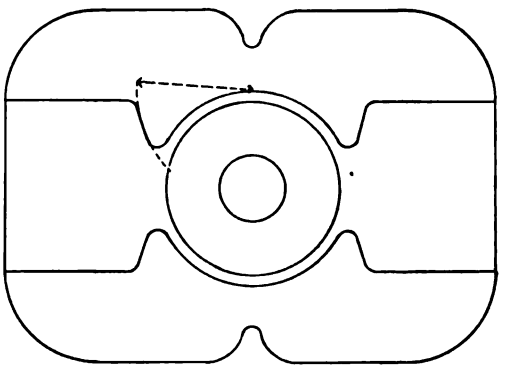


Fig. 6.

It was then almost entirely rebuilt. The armature was provided with a core that was considerably larger in cross-section, and the maximum magnetic density used in it was 11,000 lines per sq. cm. as against 20,000 used before. The lugs on the core were dispensed with, and the wires wound on the surface of the core. The poles were made of cast-iron, and fashioned to accord more nearly with those in Fig. 3. The air gap required ten thousand ampere turns to set up the magnetization through it at no load, while the armature ampere turns were 8,000 at normal output, so that series ampere turns had only to be added to counteract the action of the ampere turns on the armature that lie between the double angle of lead, to increase slightly the E. M. F. by the amount equal to the fall of potential through the armature caused by its resistance, and to compensate for the slight effect of the

pole corners that still became saturated to a limited extent for the higher outputs. It should be remembered that the magnetic leakage that takes place between the adjacent north and south pole corners, one of which is strongly and the other weakly magnetized, plays an important part in saturating thin pole corners. It is evident that unless the "armature characteristic" is a straight line as in curve I., Fig. 7, that the machine can not be made to regulate for constant potential with a high degree of refinement. The poles were again changed and shaped as in Fig. 4, when an "armature characteristic" given in curve I. was

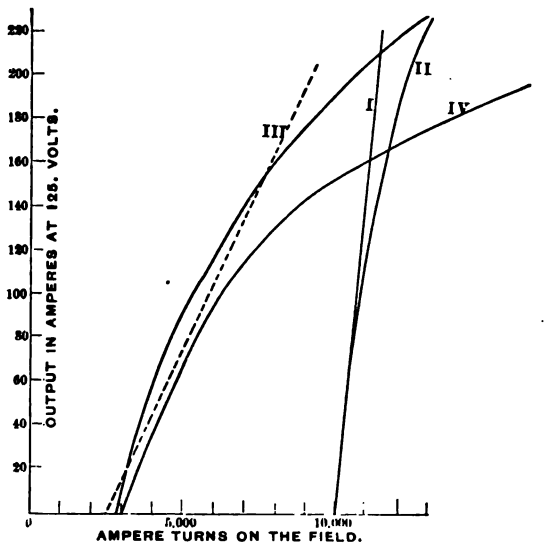


Fig. 7.

obtained, and the proper number of shunt and series ampere turns for a refined degree of regulation were readily decided upon. These experiments confirm what has been said above, and show how useless have been the attempts to diminish the air gap beyond certain limits.

It was shown in the outset how we can calculate the actual magnetic densities in the air gap for any total magnetization through the armature, for any armature current. The results of the following experiments confirm the correctness of these methods. The diagrams in Fig. 8, give the values of the magnetic density at all points of the air gap of a generator producing

125 volts at the brushes and 80 amperes. The following are its dimensions and data:

Diameter of armature core	6.25 in.
Length of armature core	12. in.
Diameter of bore of poles	7.19 in.
Double depth of air gap	.94 in.
Armature sections	50.
Turns per section	2.
Resistance of armature	.06 ohms.
Poles shaped as in Fig. 3,	
Shunt turns on field	6400.
Field current, no load, 125 volts	1.48
Field current, full load, 125 volts	2.10
Speed	1600.
Carbon brushes used without lead.	

The "armature characteristic" curved considerably, indicating that the pole corners become saturated. It is evident too that

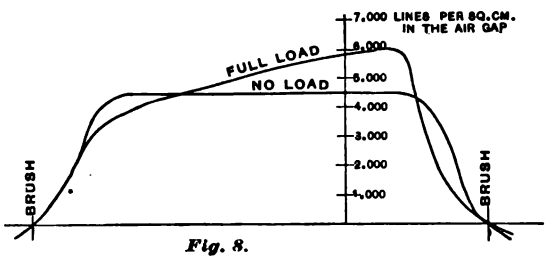


Fig. 8.

the normal magnetization in the pole corners in addition to the magnetic leakage which is greater there than anywhere else, produced saturation in all pole corners, even with no current in the armature. For at full load there were 4,000 ampere turns on the armature, while 4,000 series ampere turns had to be added to the field that produced 125 volts at no load to keep the E. M. F. the same. Therefore at full load we have the same number of ampere turns acting through the weakened pole corners as at no load, and the total amount of magnetization has only been increased 5 per cent., to compensate for the resistance of the armature conductors The conditions, however, are not the same, for there are just 4000 more ampere turns to cause magnetic leakage at the pole corners, so that on the whole the magnetization in them is increased. This increase of magnetic density in them greatly increases their magnetic resistance, for they are saturated to begin with. It is on this account that we find the magnetization under the weakened pole corners diminished, when apparently

the forces acting have not been changed. The magnetization under the strengthened pole corners through the air gap is increased more than it is diminished by the effect of the added magnetic leakage, through the 8000 additional ampere turns that act to produce magnetization by this route through the armature.

In Fig. 9 the diagrams show the magnetic action of the armature of a 10 H. P., 110 volt motor, with poles fashioned as in Fig. 6. Measurements of the magnetic leakage were made on this motor, and the results indicate that the shape given to the pole corners avoided saturation in them even at full load. The double angle of lead was almost 60 degrees. The ampere turns embraced by it on the armature were partially compensated for by 9 series turns on each of the consequent fields. The remainder of the armature ampere turns that lie between the double angle of lead served to weaken the field by just the amount required to produce

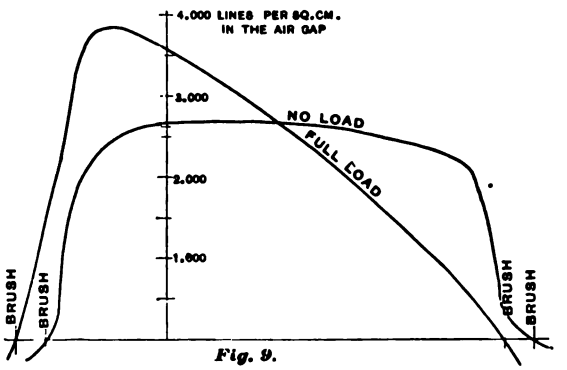


Fig. 9.

a constant speed. The following figures give additional data on this motor:

Diameter of armature core	8.33 in.
Diameter over all	9. in.
Bore of poles	9.38 in.
Double air gap	1.05 in.
Shunt turns on field	2200.
Shunt current at 110 volts	3.55
Ampere turns on field at 110 volts	7800.
Ampere turns on armature at full load	5750.
Armature sections	48.
Turns per section	3.
Speed	1200.

The ampere turns required to set up 2,600 lines per sq. cm. through a distance of 1.05 in. or 2.64 cms. in open air—

$$\frac{2600 \times 2.64}{1.26} = 5430.$$

This is the number of field ampere turns that exerted their magnetizing force between the pole faces through the armature. The ampere turns acting through the weakened pole corners are, therefore, very near zero, which is entirely corroborated by the fact that the magnetization was observed to be zero at this point. See full load curve in Fig. 9. Through the strong pole corners the ampere turns acting were the 5430 of the shunt ampere turns, + the 720 of the 9 series turns + the armature ampere turns, 5750—twice the ampere turns between the double angle of lead, 2. [.3×5750], or 3450 = 8550 which will produce a magnetic density through an air gap of 2.64 cms. depth of

$$\frac{8550 \times 1.26}{2\ 64} = 4100.$$

while the actual magnetic density measured at this point was 3950, an agreement within the possible limit of error.

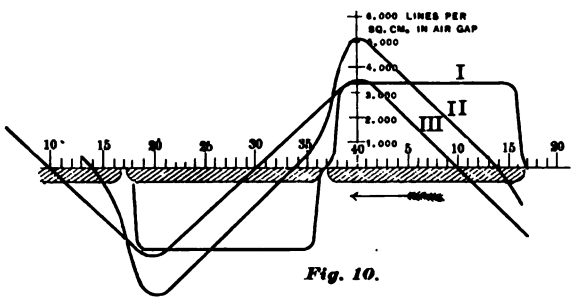


Fig. 10.

In Fig. 10 are given curves showing the magnetic performance of an armature, with its conductors laid in deep narrow grooves, as shown in Fig. 12. The clearance on each side was one-sixteenth of an inch, making the double air gap one-eighth of an inch. Additional dimensions are as follows:

Diameter of the armature core	6.	in.
Length of armature core	6.	in.
Resistance of armature	.34	
Number of sections	64.	
Turns per section	8.	
Output, amperes	20.	
" volts	100.	
Speed	1800.	

Curve 1, in Fig. 10, shows the distribution of magnetization at 112 volts, no current, a speed of 1800, and a field excitation of 2600 ampere turns. Curve II shows the magnetic distribution for an output of 97 volts and 24 amperes, at a speed of 1800, and a

field excitation of 2600 ampere turns. Curve III shows the magnetic distribution at an output of 40 volts and 20 amperes, at a speed of 1800, with a field excitation of 750 ampere turns. This same excitation when the armature furnished no current, produced an E. M. F. of 48 volts, at a speed of 1800 revolutions. The poles were shaped as in Figs. 11 and 12, but modified as explained below. In making these experiments carbon brushes were used, and their position maintained at the normal diameter of commutation. An average magnetic density in the air gap of 3400 lines per sq. cm. was required to produce an E. M. F. of 112 volts. The grooves on the armature in which the conductors were placed, occupied one-half of the armature surface, so that the actual mag-

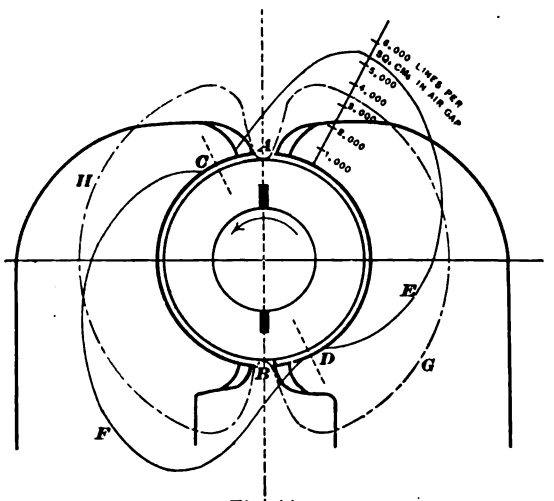


Fig. 11.

netic density in the air gap was 1.8 times this average magnetic density. The ampere turns required to set up this magnetic density in the air gap were

$$\frac{.125 \times 2.54 \times [3400 \times 1.8]}{1.26} = 1520,$$

which is the number of field ampere turns whose magnetizing force is impressed between the pole surfaces through the armature, when it furnishes 112 volts and no current. The ampere turns on the armature at a current of 24 amperes are

$$\frac{2 \times 64 \times 3 \times 12.}{2.} = 2300.$$

as against 1520 impressed by the field. Under these circum-

stances the magnetism under the weakened pole corners is reversed as is also clearly indicated by curve H, in Fig. 10, or the curve G, E, D, F, C, in Fig. 11. This curve also shows that the magnetic density under the strong pole corners was 5,100 lines per sq. cm. Now in building this machine, six longitudinal slots, $1\frac{1}{4}$ inch. deep, were cut in each pole immediately back of the surface, which enables us to be sure of the exact densities in the pole corners for a given distance. For a depth of $1\frac{1}{4}$ inches immediately back of the pole faces, these slots took up one half of the cross-section of the poles. Then a density, therefore, of 5100, really means a density of 10,200 or a strong saturation for a distance of $2\frac{1}{2}$ inches

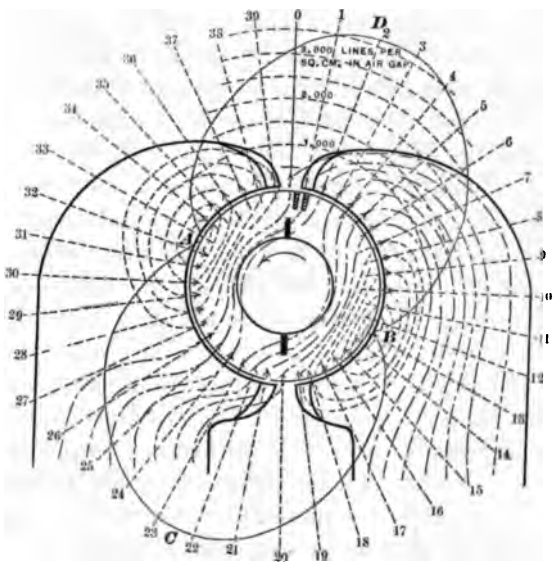


Fig. 12.

in the cast-iron of the poles. The magnetizing force required to produce 10,200 lines per sq. cm. through cast-iron is 200 per centimetre length. Therefore the ampere turns required to establish this density through $2\frac{1}{2}$ inches are

$$\frac{2.5 \times 2.54 \times 200}{1.26} = 1{,}000.$$

The total number of ampere turns acting to produce magnetization through these strong pole corners was the sum of the field ampere turns that impressed magnetizing force from pole face to pole face through the armature, and the ampere turns on the armature covered by the poles. The poles covered approximately 85

per cent. of the armature surface, making this value: 1500 × [2300 × .85] = 3400. Of this number, as was just shown, 1000 were utilized in producing the magnetic density of 5100, through the saturated portion ofthe poles. The remaining 2400 ampere turns exerted their magnetizing force in producing the average magnetic density of 5100 through the air gap, and affords another opportunity of checking these ideas of the action of the armature on the field. For the ampere turns required to set up an average magnetic density of 5100, through the air gap under consideration were:

$$\frac{[1.8 \times 5100] \times .125 \times 2.54}{1.26} = 2300.$$

which checks with the above value as well as could be expected. When the armature furnished 24 amperes, the E. M. F. at the brushes was 97 volts, while with no current it was 112 volts. Of this drop of 15 volts, 8 are accounted for by the resistance of the armature, and the extra seven were caused by the saturated pole corners. By operating this same machine at an E. M. F. at which the pole corners could not saturate with normal output of current, we have demonstrated for us in a very striking manner that the armature ampere turns can not change the total magnetization established through the armature by the field when the pole corners do not saturate. The E. M. F. with the armature current at zero, was brought to 48 volts with a separate field excitation of 750 ampere turns. Then when the armature was allowed to furnish 20 amperes the E. M. F. at the brushes dropped to 40 volts. Of this drop of 8 volts, 7 were produced by the resistance of the armature. Yet the field is powerfully distorted by the armature current, as may be seen by reference to curve III, Fig. 10, or the curve A, D, C, B in Fig 12. Even with this very great rearrangement of the magnetization produced by the armature current, the total magnetization set up by the field is practically unchanged. The difference of potential on the commutator between the points A. B., Fig. 12, was observed to be 72 volts. This excess of E. M. F. over that which was produced at the brushes, the figure shows clearly to be due to the magnetization produced by the armature through itself and the strengthened pole corners. The points where the field is zero are at A, B. They mark the diameter through which the ampere turns encountered on the armature are just equal and opposite in action to the ampere turns of the field that impress a magnetizing force between the pole faces through

the armature. A simple computation will show that this is true. The field ampere turns that impress magnetizing force between the poles when the armature produced an external E. M. F. of 48 volts are

$$\frac{1520 \times 48}{112} = 650.$$

The ampere turns on the armature opposed to the magnetization set up by the route A, B. are $\frac{12}{40}$ of the total number of ampere turns on the armature, [see Fig. 12,] or

$$\frac{12 \times 64 \times 3 \times 20}{40 \times 2} = 576.$$

This is a fair agreement when we consider the accuracy with which the original data may be determined.

Mr. Esson in his valuable paper, above referred to, discussed the requisite features for a generator for constant current, with closed coil armatures, in which regulation is effected by shifting the brushes. He stated that the field should be uniform at all points, under the poles, and that the armature core should be saturated. These statements are a little misleading. The magnetizing force impressed by the field ampere turns must be uniform at all points between the pole faces. This is accomplished by proportioning the poles so that the strongly magnetized pole corners will not become saturated when the brushes have their extreme position for the development of the highest E. M. F. that the machine is to produce. The air gap is made of such a depth that the ampere turns required to set up the magnetization through the armature, without current, and for the production of the highest E. M. F. that the machine will be called on to give, shall be a little more than the armature ampere turns when it furnishes its normal current. Then as long as the brushes are kept under the pole faces, the non-sparking point will be wherever the brushes are placed. This will be the case whether the armature is or is not saturated. A practical demonstration is found in the following experiment: A Siemens and Halske dynamo with magnet and armature cores, whose shape and dimensions are shown in Fig. 5, was used.

Length of armature core	7.25 in.
Number of armature sections	56.
Turns per section	6.
Revolutions	1000.
Output in volts	50.
" " amperes	80.

The field was separately excited with 4000 ampere turns on each of the sets of consequent poles. Regulation could then be effected for a constant current in the armature, of 22 amperes, by shifting the brushes from no E. M. F. to 35 volts without the slightest sparking even when metallic brushes were used. Within this limit the pole corners did not saturate. The field cores were wrought and the yokes cast-iron. When the armature circuit was broken, it was found that the field excitation of 4000 ampere turns produced an E. M. F. of 50 volts. The magnetic density in the field cores, including leakage, was only 11,000 lines per sq. cm. Therefore, of the 4000 ampere turns on the field, not more than 200 were applied in setting up the magnetization from pole face to pole face through the field cores. It is safe to assume, then, that of these 4000 ampere turns, 3800 were active in producing a magnetizing force impressed uniformly over the pole faces through the armature. This same value is obtained by the method adopted in the previous cases. That is by calculating the magnetic density in the air gap when 50 volts were developed, and then deducing the number of ampere turns required to establish such a magnetic density through a $1\frac{1}{2}$ inch air gap. As to the armature, when it produced 22 amperes its ampere turns numbered

$$\frac{2 \times 56 \times 6 \times 11}{2} = 3700$$

or an excess of 100 ampere turns impressed by the field over and above those on the armature. As long as this same number of ampere turns was maintained on the field it was not possible to regulate for a constant current of a lower or a higher strength without sparking. The impressed field ampere turns are in excess of the armature ampere turns by that amount which is just sufficient to produce a weak positive field that will reverse the current in the coil when its terminal bars at the commutator pass under the brush. When regulation is effected by this means it is seen that all pole corners are alike magnetized and at the centre of the pole faces the magnetization is zero when the machine is short-circuited. At full output, at the highest E. M. F., the magnetization under the one set of pole corners is almost zero, and under the other set it is at the maximum value that is ever obtained. In a generator of this type when the poles are made stout enough at all points, the total amount of magnetization through the armature, at all loads, will remain at a constant value.

When used for operating long arc lamps, a "drooping characteristic" can better be obtained by saturated field cores than by saturated pole corners and armature core.

What has been said of dynamos applies equally well to motors. The only difference being that the non-sparking points are on opposite sides of the diameter at which magnetization is zero in the air gap. This is due to a well known cause. In a motor, the armature rotates in an opposite direction when field and armature currents remain the same as in a dynamo. The E. M. F. of self-induction caused by the reversal of the current in a circuit has not changed, while the E. M. F. developed in the coil by the field has changed sign with the change of the direction of rotation. The result is that the reversal of the current in an armature section must take place in a weak field of an opposite sign in a motor from what it does in a dynamo, when sparking is to be avoided entirely.

The action of the current in the armatures of multipolar dynamos and motors will be the same as that found for two pole machines.

Discussion.[1]

The Chairman [Vice-President T. D. Lockwood]:—Gentlemen, you have heard Professor Ryan's paper read, and I am sure you have found it replete with interest and very instructive. Some parts at least, I doubt not, will form a substantial basis for discussion, and the paper is not so long but that we can discuss it fully and freely in the time at our disposal. I see that our worthy Past President, Professor Anthony, is here this evening, and I am sure we should be very glad to hear from him.

Professor Anthony:—Mr. President, as this is the first I have seen of the paper, I hardly feel prepared to enter upon a discussion of it, without having a little more time to think it over. I should like to have had this paper a few days beforehand, in order to feel prepared to enter into a discussion of it. There are a great many new things brought out here—at least they seem new to me. The matter is brought out in a somewhat different light from what I have thought of it before. I should therefore beg to decline to enter into any discussion of it at present.

The Chairman:—As a basis for remarks from some member, I would like to call attention to a point where Professor Ryan seems to differ somewhat from the opinion of Mr. Esson. The author says that "Mr. Esson, in his valuable paper above referred to, discussed the requisite features for a generator for constant current, with closed coil armatures, in which regulation is effected

1. By Messrs. W. A. Anthony, Wolcott, Mailloux, Lockwood and Tesla.

by shifting the brushes. He stated that the field should be uniform at all points, under the poles, and that the armature core should be saturated. These statements are a little misleading. The magnetizing force impressed by the field ampere turns must be uniform at all point between the poles' faces."

It seems to me that there is some foundation for remarks there, from any member who has made that point a study.

MR. TOWNSEND WOLCOTT:—It seems to me that Prof. Ryan simply explains what Mr. Esson means there. Mr. Esson says that the field should be uniform at all points, and Prof. Ryan states that that part of the field which is due to the field winding should be constant. That is, it might indicate total resultant field in the air gap as due to the field, and armature windings—that should not be constant, but the component due to the field windings should be constant. That is what Prof. Ryan means, and I think that is what Mr. Esson meant.

PROF. ANTHONY:—The difference in that case between Prof Ryan and Mr. Esson seems to be that while Mr. Esson says the armature core should be saturated, Prof. Ryan says this would be true whether the core is saturated or not.

THE CHAIRMAN:—Mr. Mailloux, will you favor us?

MR. C. O. MAILLOUX:—Mr. Chairman, I am somewhat in the same position that Professor Anthony is in, respecting this paper. I do not feel that I understand the paper sufficiently to be able to discuss it intelligently. It contains much matter which requires very careful analysis and digestion to enable it to be discussed properly and intelligently. There are probably some statements to which we might take exception. I think it would be a very much better and more profitable discussion if we had a chance to study the paper, and if Prof. Ryan himself were here to answer questions when the paper is discussed. The whole subject of armature reactions is among the most difficult and puzzling in electrical engineering. There can scarcely be a more perplexing problem, because we have to deal here with the phenomenon of "action at a distance," which has engaged and baffled the efforts of all the great scientific thinkers of the world since the days when Newton pondered over it and made some steps toward a theory which he applied to actions at a distance among the heavenly bodies. During the last few years there has been a great deal written on what might be called the "physics of the air gap" though I believe that the true physical theory of the air gap has yet to be written. Even with all the theory which we have, and while paying proper deference and respect to the very able work of such men as Hopkinson, Swinburne, Esson and others—I can but believe that we have not, after all, much more than working hypotheses; that we have, so to speak, only provisional theories, which are good so far only as they cover the facts now known to us. The truth is, we do not yet know all the facts pertaining to the physics of the air gap, and it will

probably be some time before we understand them all. Though I will not undertake the discussion of this paper, I would refer to one point in connection with what is said near the top of page 454: "When the armature ampere turns are equal to the ampere turns on the field, whose magnetizing force is impressed between its pole faces through the armature." It is quite evident from the sentence which immediately follows that the author wishes to imply what is sometimes spoken of and what is generally understood, as difference of magnetic potential. Mr. Swinburne, I believe, has given it that name, and as we have the exact analogue of it in relation to the electric circuit, I see no reason why we could not use the term in relation to magnetic circuit, and thus express the author's meaning at least as clearly and more succinctly. It is mathematically the line integral of magneto-motive force for that part of the circuit. It means, so to speak, the magneto-motive force lost in the air gap; or the whole magneto-motive force minus the portion that is lost in the field reluctance itself.

The much-mooted question as to the length of the air gap and its relation to the phenomenon of sparking, of course, forms one of the principal subjects that the author has discussed. I have given that subject a great deal of attention in my professional work, and I have taken great care to scan and to canvass the literature on the subject. I find that there is considerable divergence of opinion, and that while theorists are telling us that we should not make the air gap too small, and that we must have a certain minimum of resistance there, yet machines are being made which are very successful, not to say most successful, in which the air gap is exceedingly small The Oerlikon Dynamo Works, in Switzerland, is perhaps one of the most successful firms building dynamos in the world to-day, and they are noted for the extremely small air gap used in their dynamos, the armature iron being perforated near the edge and the wires passed through the perforations near the periphery of the armature, so that the air gap may be, and is, very small. Mr. Ravenshaw in England has, I believe, cited some experiment with a given machine which he first tried with a very small air gap. He then bored out the field and tried the experiment over again, maintaining the same magnetic density through the entrefer. He found, if I quote him correctly, that it did not make any practical difference so far as the limiting output of the machine within which sparkless running occurs, was concerned. One would conclude from this that it does not make very much difference what is the length of the air gap if certain other apparently equally important factors receive due consideration in the design. Among the factors which a comprehensive theory of armature reactions should include, in my opinion, and which are somewhat neglected by the author, are the number and length of armature reactions, the width of pole pieces, the mode of winding, etc. I regret, indeed, that I

have not had a chance to thoroughly analyze the paper. It certainly discusses one of the most important subjects which could be brought before this Institute, because it relates to a problem of the greatest importance, and regarding which any new knowledge must be of great value to the designer of electrical machinery. It is certainly to be hoped that a good discussion will be had upon it at some future date. I think the paper is well worthy of the attention of the ablest and most competent members in our midst.

PROF. ANTHONY:—Mr. President, I would say one or two words further in regard to this. I feel exactly as Mr. Mailloux does—that we ought to have some opportunity for a discussion of this paper after we have had some time for the study of the paper itself. There are such a multitude of details connected with the action of the armature in a dynamo that it will take very considerable study before we can discuss the paper very intelligently. The length of the air gap, the form of the pole pieces, the form of the pole corners, the relation of the pole cores, of the field cores, the number of ampere turns on the field cores, the winding, and I don't know how many dozen of other things will affect the action of the armature, affect the sparking and the relation of electromotive force, whether constant or not, and all these different things must be considered and their effect taken into account. It seems to me a pretty difficult thing for any one to consider off-hand. I should like to say one word in reference to the width of the air gap here. It was something of a surprise to me to have it implied, as it seems to be in this paper, that the air gap must have somewhat of a magnitude in order to make the dynamo work well. My idea has always been that we must make the air gap as small as possible, and the smaller we could make it the better the dynamo would work. I do not know that I have ever seen anything in my experience that would indicate the necessity of making the air gap any larger than was necessary for getting in the wires and in fact, in a machine that I have recently constructed, we made an armature two feet in diameter for the very purpose of getting the wire in one layer and getting the air gap only one quarter of an inch, with about 100 amperes normal output. When the generator was running street cars, and the current was jumping from 0 to 200 amperes and back continually, it was almost impossible to tell by watching under the brushes when the generator was doing work. The creaking of the engine, etc., indicated it and a man could imagine that he saw a little sparking when he heard the engine working. They were carbon brushes.

MR. MAILLOUX:—I am in accord with Prof. Anthony on the point of the air gap, because I have not only advocated, but I have practised short air gaps for several years.

THE CHAIRMAN:—One moment. By air gap here we understand of course the place between the iron of the armature and the iron of the pole pieces

MR MAILLOUX :—Yes, sir, which the French call entrefer.

THE CHAIRMAN :—It is not possible that Professor Ryan is speaking of the nearest approach of the two poles.

MR. MAILLOUX :—No, sir, that is called the space between the poles.

THE CHAIRMAN :—I ask because I do not think air gap is the very best term.

MR. MAILLOUX :—I have found practically the same result as found by Prof. Anthony. I have never thought that a short gap necessarily made a sparking machine, if rightly designed. I have in mind now some railway motors which were run with an air gap of scarcely a sixteenth of an inch, and having armature teeth at that; and the machine was quite sparkless at any load for which it was designed. According to theory one would expect such a machine as that to spark very badly. My own experience and the results reported from Europe, particularly from the house that makes the Oerlikon machine in Switzerland have made me less afraid of a small air gap than I used to be.

MR. WOLCOTT :—I would like to ask Mr. Mailloux in regard to that machine, if the teeth were saturated.

MR. MAILLOUX :—No, sir. I examined that point to-day for another purpose. The teeth were not any more saturated than the armature core. The armature core itself had a very low magnetic density. I think that the highest density ever reached through the armature core did not exceed ten thousand lines per square centimetre and that at the normal run it was scarcely six thousand.

MR. WOLCOTT :—It makes it all the worse for the small air gap theory. If the density were very high in the teeth, it might serve the same purpose as the resistance in the air gap, but if the density were low in the teeth it seems to knock the theory all to pieces.

MR. MAILLOUX :—There have been cases where I have been compelled to use high magnetic density in the Pacinotti teeth, so high a density that the permeability fell down to about five; yet I have not noticed that it had any material influence on the sparking.

MR. J. STANFORD BROWN :—The value of the paper has been properly emphasized, likewise the originality of treatment of the subject. As none of us seem particularly prepared to-night to discuss the subject, would it not be well to have it announced that this paper will be more thoroughly discussed at the next, or the following meeting, and would it not be a wise plan, in the case of many of those engaged in designing machinery, and who know more about it than any others, to send this paper to them with the request that, if they can not be present to aid in the discussion, they would be good enough to send in writing so much as they might be willing to add to the subject? This might result in bringing out a great deal of information we can not otherwise get.

THE CHAIRMAN :—It may not be out of order to state that in the card which I received notifying me of this evening's meeting, the subject was mentioned with the intimation if I was particularly interested therein, I could receive a copy by sending for one, but as I supposed there would be a great many here more interested in air gaps than I was, (since my business relates to the very smallest dynamo in the world in which I have always found that the smallest air gap possible was the best for the machine), I did not take the trouble to send for the paper. Perhaps, like other gentlemen, I should therefore plead my inability to discuss the subject on that score. I think we may reasonably regret that Professor Ryan is not here with us this evening to answer questions. I should have wished for myself that the author of the paper had been a little more particular to classify the different machines in question, for while he speaks in a general way at first, and subsequently speaks of a constant potential machine, it is rather difficult to tell what machine he is referring to most of the time, until on the last page but one, where he comes to speak of "Mr. Esson's valuable paper" to which I referred before. He speaks there, of a generator for constant current, by which I suppose he means what we call a series machine with closed coil armatures, by which regulation is achieved by adjusting the brushes. I have a very distinct idea that sparking in dynamos is largely due to a lack of harmony between the short-circuited current, as an individual coil passes under a brush, and the current in that side of the multiple coil armature which the short-circuited coil is approaching. And it has further appeared evident to me, that if we could produce at the moment when the front segment of the commutator leaves the brush—if at that moment we could produce a zero, by opposing the short-circuited current against the current to which the short-circuited coil is about to be introduced, we should have a perfect neutralization, and in consequence, an absence of sparking. I am well aware that there has been a kind of hazy doctrine circulating, or at all events existing among electricians, that the sparking upon a change of current, and when brushes were used to compensate for that change of current, is largely due to the fact that the neutral line had moved forward, and that therefore the brushes would have to be moved forward to catch up to the neutral line, and when the brushes did catch up with the neutral line, the sparking would cease to exist. But when you pursue that theory to its lair you find it is moving in a circle. It may not be out of order to recount an experiment I witnessed showing that in reality as the current in a series machine changed under a difference in load, the neutral line remained stationary as long as the brushes were not moved. For example, suppose a machine was working on a load of forty arc lamps, and some ten of those should be cut out —the load would be very much less, the current would be much stronger—and in a machine furnished with an automatic regula-

tor, the regulator would move forward to cut down the electromotive force developed of course, in bringing the current down to its constant figure once more. It was supposed by many that the neutral line of the machine moved forward when the current changed. That is, when the current increased, the neutral line moved forward. It hardly seemed to me that it was a possible thing, seeing that the current was strengthened not only in the field helices, but also in the armature, the machine being a series machine. A single wire was rigged around the armature. One end was attached to a fixed ring, while the other end was attached to a movable commutator, and the circuit of this wire included a telephone receiver. When the machine was working on a full load we found a place where there was a zero of current. It was absolutely on the neutral line of the machine as it was working then. By the neutral line I mean the moment when a given coil passes over from the influence of the pole upon one side, to the pole upon the other—when it is at the exact point between the two—that is, when it ceases to cross lines of force in one direction and begins to cross them in the other direction. We found that point, and found that the telephone gave forth no sound at all upon changes of resistance and consequent changes of current. Just as long as the brushes were unmoved the telephone gave no sound, no matter how many lamps were put out or how much the current was changed, showing that the neutral line did not move. But when we changed the position of the brushes and moved them forward to compensate for the increased current, we found that there was a change and that the neutral line did not move forward as it was supposed to do, but actually moved backward. Now, that was the result of absolute experiment, and I think when we reflect upon it we will find that experiment is in accord with the best theory, and therefore I think the gap has such a relation to sparking in the constant current machine only in so far as it may modify the form of the pole pieces and, instead of using the phraseology here, I should rather be inclined to use the phraseology that "the requisite feature for a generator for constant current on closed coil armature in which regulation is effected by shifting the brushes, is that the field should be uniform throughout the range of movement of the brushes."

Mr. Nikola Tesla:—I have had excellent opportunities to study the importance of the air gap, particularly in some types of my motors in which the armature coils are closed. Such a motor is of course a transformer and, following the analogy, the first idea was to secure in it as good a closure of the magnetic circuit as practicable. In the early experiments I constructed motors in which the air gap was often as low as a sixty-fourth of an inch at the start and was gradually increased. This was the result arrived at: Up to a certain point the diminution of the air gap would act very favorably; then the further approach

of iron or the reduction of the air gap would begin to be a great drawback, and it has always been my idea that the real cause of that was the unsymmetrical position of the armature to the field, and the impossibility of making the motor so mechanically perfect that the pull on both sides would be equal. Some time ago I did some work on unipolar dynamos with iron discs and I found that the lateral thrust was very difficult to overcome and it was impracticable to work with very small air gaps, a fact which of course is well known. In the results presented by Professor Ryan there is a point which he has not considered, and which, if I may be permitted to say it, I think ought to have been considered, because it is of enormous importance—so important that it radically changes the whole result, and that is this: In a motor or generator in which the polar faces and the armature core both are smooth, we can reduce the air gap indefinitely, and the smaller it is the better, and in many experiments I have found that we cannot reduce it far enough, disregarding mechanical difficulties. But directly we put projections on the field, as, for instance, Professor Ryan has done (he mentions an experiment in which there were six slots across the field), we cannot work any more with a very small air gap. You see the reason plainly enough. The putting on of these air gaps brings the whole investigation into a different field. If the polar faces be smooth, no matter how close you approach the iron to iron, the armature undergoes a gradual change, but directly you have the grooves, each part of the armature surface has to get out from the very intense field into a weak field and then from the weak into an intense field, and so on repeatedly—six times, as in Professor Ryan's experiments. I have made a great many experiments which confirm that. You can take a smooth field, put a laminated armature without projections in it and work with a certain magnetization, revolving the armature at a given speed. It will not get hot to a high degree. Now put in grooves through the field, and you will find at once that with far less magnetization than before, the armature will get intensely hot. The reason is, that you put each part of the armature through a much more sudden and greater total change, and increase the frictional and current losses. That is the reason also that in the case of the grooves there is a certain increased loss in the voltage, attributed by Professor Ryan wholly to a different cause.

The Chairman's remarks in regard to Mr. Esson's valuable paper, suggest to me a subject in another line of work which I have been pursuing. Mr. Esson pointed out certain requisites of constant current generators with closed coil armatures which practice would not seem to confirm. On this subject I think that I will not risk much in predicting that in a comparatively short time most of the present constant current machines will be replaced by machines of a more modern and improved construction. The old types of these machines have been built by ingeni-

ous men who, rather instinctively, hit upon the right thing—right at that time, but not right now. The main object in these machines is to maintain a constant current in the most efficient and simple manner. Some time ago I had occasion to say a few words —I think in a paper—in regard to this matter. I have constructed such machines in accordance with my view, which is to produce a great rate of change in the current and to replace the ohmic resistance required for steady working, by impedance. In this manner you can construct an arc light machine with only one single layer, no matter how large it is, and you can make it maintain a constant current, within two or three per cent. without any external regulator. One of the ways of obtaining this result is the following: Take, for instance, a two pole machine, selecting a type which allows you to use an armature of the greatest possible diameter. By all means, make the armature as large as possible. Then, instead of making the pole projections of the field smooth, make them with grooves parallel to the armature shaft. The more you make, in fact, the better it is up to a certain point. Then wind the armature symmetrically with reference to the pole projections or grooves, so that you get a great rate of change in the current. Now then, you have a direct current which is commutated just the same as in the Gramme machine, but as to the rate of change it is equivalent to a rapidly alternating current, and of course you increase the impedance through the whole circuit to the extent that you can do away with the great ohmic resistance in the circuit, and you can make the impedance and the armature reaction so great that no matter what the load is, the current is the same. Such a machine has numerous advantages, not the least of which is that it is more efficient, and then again it is so easily repaired, only one layer being required. In all alternating generators, almost without exception, the armatures are built with one single layer of wire The arc light machine must also be built with single layers and the right way to do this is to provide the pole pieces with projections and wind the machine in such a manner that a current of the requisite character is obtained. In regard to the shape of the pole pieces, I had opportunity during the year 1885 to make some observations I then worked at what is called the third brush regulation. It is a principle very interesting, but not very reliable. It may give a wonderful result for a short time, but when you want to show it to the stockholders it cannot do very much. It has been original with me. There have been some other claimants—there are always claimants. But to return to the subject: One of the difficulties encountered in the third brush regulation was the excessive sparking of the third brush. To overcome this, the principal requisite was that the auxiliary brush should not bear on active coils. I then resorted to a means which was obvious. I put under every leading pole piece a good sized groove, which made the coils on each auxiliary brush comparatively inactive, so that

the auxiliary as well as the main brushes would bear upon inactive coils. In this manner I obviated the sparking, but the machine did not look particularly pleasing. Mr. Westinghouse, when he first saw an experimental machine, which later was built at his works, said, looking through the holes, "Mr. Tesla, I like this machine very much. What I like about it best is that one can see through it." Although these machines have not proved a practical success, yet experience with them led to useful results. In the preceding remarks I have not desired to detract in the least from the merit of the opinions expressed by Mr. Esson, nor from those of Professor Ryan, whose investigations are always systematic.

THE CHAIRMAN:—Do you make your suggestion as a motion now, Mr. Brown?

MR. BROWN:—Yes, sir; I make the motion that it be definitely announced that there will be further discussion of this paper at the next meeting or the next following, as may seem best to the Chairman to-night, and that the Secretary be asked to forward copies to such members as he judges are more particularly conversant with the subject, with the request that they send us letters.

THE SECRETARY:—I cannot always tell who are interested. I do my best. But this paper will be published in all the electrical journals at once.

MR. BROWN:—It may not be necessary to send copies. It might be wise to ask for some letters.

MR. MAILLOUX:—I second the motion.

THE CHAIRMAN:—It is moved and seconded that there be further discussion announced upon this paper upon some subsequent meeting.

MR. BIRDSALL:—I think it would be well to endeavor to have Professor Ryan here at the meeting when the paper is discussed.

THE CHAIRMAN:—I do not know how we can get him here.

MR. BIRDSALL:—The Secretary might open correspondence with him. We might have a collation; that generally brings the members.

THE SECRETARY:—I think it is proper to say that Professor Ryan was given charge of installing an underground plant at Cornell, and he said that he could not leave a single day. He was very much disappointed in not being able to attend in person.

THE CHAIRMAN:—Then I will put the motion in this way: It is moved and seconded that further discussion of this paper be postponed until some subsequent meeting to be announced hereafter, when we shall hope to have Professor Ryan with us.

[The motion was carried.]

Mr. Wetzler read some correspondence with respect to proceedings of the Frankfort International Electrical Congress.

THE SECRETARY:—The following is a list of associate members elected by Council since the May meeting.

June 26th, 1891.

Name.	Address.	Endorsed by
BARNARD, JOHN H.	General Manager, Asheville Street Railway Co., and Asheville Light and Power Co., Asheville, N. C.	M. J. Wightmun. H. F. Parshall. J. G. White.
CRAM, HENRY B.	Treasurer, Bernstein Electric Co., 185 Prospect St., Cambridgeport, Mass	Theo. Stebbins. Geo. W. Davenport. S. C. Peck.
DAME, FRANK L.	Superintendent, Vancouver Electric Railway and Light Co., Vancouver, B. C.	J. P. B. Fiske. I. E. Winslow. Chas. R. Cross.
EMERY, CHARLES EDWARD,	Consulting Engineer, 22 Cortlandt St., New York.	Edward Weston. Thomas A. Edison. Frank J. Sprague.
FAY, THOMAS J.	Maine Electric Improvement Co., 136 Liberty St., New York.	Harry Alexander. W. A. Rosenbaum. M. C. Sullivan.
FISH, WALTER C.	Manager, Bernstein Electric Co., 620 Atlantic Ave., Boston, Mass.	Edwd. Blake. Geo. W. Davenport. S. C. Peck.
GANS, JOHN L.	Superintendent and Electrician, The Electric Company, Connellsville, Penn.	J. G. White. R. W. Pope. T. C. Martin.
HADLEY, WARREN B.	Supt. Wiring Dept., Edison Electric Illuminating Co., 431 Fifth Ave., New York.	L. Stieringer. T. C. Martin. S. S. Wheeler.
HEALY, LOUIS W.	The Wightman Electric Co., 1205 Marion St., Scranton, Pa.	M. J. Wightman. E. L. Nichols. Ernest Merritt.
KELLOGG, JAMES W.	Assistant to District Engineer, Edison General Electric Co., 140 E. 27th St., New York.	M. C. Sullivan. T. C. Martin. R. W. Pope.
LUQUEE, THATCHER T. P.	Asst. in Surveying and Practical Mining, and Student in Electrical Engineering, Columbia College, N. Y. and Bedford, N. Y.	M. I. Pupin. F. B. Crocker. W. H. Freedman.
ROSEBRUGH, THOMAS REEVE	Lecturer in Electrical Engineering, School of Practical Science, Toronto, Ont.	T. D. Lockwood. R. W. Pope. T. C. Martin.
TAINTOR, GILES	Assistant Electrician, N. E. Telephone and Telegraph Co., 7 Walnut St., Boston, Mass.	I. H. Farnham. T. D. Lockwood. Chas. R. Cross.
WIRT, HERBERT C.	Electrician, Marine Dept. Thomson-Houston Electric Co., 12 Millmont St., Roxbury, Mass.	S. C. Peck. Theo. Stebbins. G. W. Davenport.
WOODWARD, FRANCKE L.	Student in Electricity, 49 Grand St., Albany, N. Y.	R. W. Pope. T. C. Martin. Jos. Wetzler.

Total 15.

Council Meeting, Sept. 22d, 1891.

CALDWELL, FORDYCE S.	Proprietor Western Electric Construction Co., 503 Delaware St., Kansas City, Mo., and 151 Henry St., Brooklyn, N. Y.	E. R. Weeks. Maurice E. Bates. J. H. Vail.
CARTWRIGHT, FRED'K G.	Electrical Engineer, and Agent Fort Wayne Electric Co., 35 New Montgomery St., San Francisco, Cal.	Sidney B. Paine. H. L. Lufkin. E. E. Ries.
CHENEY, W. C.	Electrical Engineer, Willamette Falls Electric Co., Portland, Oregon.	O. P. Loomis. P. S. Malcolm. E. F. Peck.
FOOTE, CHARLES W.	Agent, Railway Dept., Thomson-Houston Elec. Co., Society for Savings Bldg., Cleveland, O.	S. D. Nesmith. E. E. Higgins. O. T. Crosby.
HALL, JOHN L.	Manager Western Union Telegraph Co., 300 Market St., Wilmington, Del.	C. R. Van Trump. J. W. Lattig. Wm. A. Mosscrop.
HAMILTON, WILLIAM H.	Dealer in Electrical Supplies, 200 State St., Albany, N. Y.	R. J. Pratt. D. H. Bates. Wm. C. Miller.
HOSFORD, HENRY H.	Electrical Engineer, 1009 Society for Savings Bldg., Cleveland, O.	S. D. Nesmith. E. E. Higgins. O. T. Crosby.
LAND, FRANK	606 Salina St. Syracuse, N. Y.	Harris J. Ryan. Ernest Merritt. Edw. L. Nichols.
MACMULLAN, ROBERT HEATH,	Treasurer and General Manager, Brush Electric Lighting Co., Lafayette, Ind.	Louis Bell. Edward Caldwell. W. J. Johnston.
MARPLE, LUCIUS E.	Electrician, United Electric Railway, Nashville, Tenn., and Wollaston, Mass.	W. H. Blood, Jr. Geo. F. Curtiss. J. P. B. Fiske.
MORDEY, WM. MORRIS	Electrician, Brush Electrical Engineering Co., 34 Montserrat Road, Putney, London, Eng.	T. C. Martin. Joseph Wetzler. G. M. Phelps.
MORSS, EVERETT.	Electrician, Simplex Electrical Co. 297 Beacon St., Boston, Mass.	F. A. Pickernell. A. F. Mason. S. C. Peck.
PATTISON, FRANK A.	Firm of Pattison Bros., Consulting and Constructing Electrical Engineers, 135 Broadway, New York City.	J. C. Chamberlain. J. T. Marshall. Wilson S. Howell.
ROGERS, EDWARD H.	Patent Lawyer, firm of Pope and Rogers, 15 Wall St., New York City.	Edward Weston. F. L. Pope. Jos. Wetzler.
RUTHERFORD, W. M.	Electrical Engineer, Thomson-Houston International Electric Co., 620 Atlantic Ave., Boston, Mass.	F. C. Bates. H. G. Reist. Franklin Sheble.
SCHLOSSER, FRED. G.	Superintendent of Electric Dept., Laclede Gas Light Co., 1801 Washington St., St. Louis, Mo.	F. R. Jones. Harris J. Ryan. C. R. Van Trump.
WHITE, GEO. MONTAGU	Agent for West Indies, Thomson-Houston International Electric Co., Kingston, Jamaica, W. I.	G. W. Davenport. T. C. Martin. Joseph Wetzler.

Total, 17.

Transferred from Associate to Full Membership, June 26, 1891.

SMITH, JESSE M. Consulting Engineer, in Mechanics and Electricity, 36 Moffatt Block, Detroit, Mich.

THE SECRETARY:—The report of the Committee on Units and Standards which, as I understand from Mr. Wetzler, includes a subject brought up before the Frankfort Congress, will probably be submitted to the Institute for discussion either at the October or the November meeting. The report was referred to the Committee on Papers and Meetings, by Council, with the request that it be brought before the Institute for discussion at an early day.

[Adjourned.]

[REPLY TO THE ABOVE DISCUSSION COMMUNICATED BY PROF. HARRIS J. RYAN.]

I have been understood to advocate that the air gap in direct current dynamos should not be made small. I have not desired to advocate either long or short air gaps. I have designed a direct current constant potential dynamo that had the smallest possible air gap that can be provided. The armature conductors were placed under the surface, and the depth of the air gap was just sufficient for mechanical clearance. The magnetic density throughout the armature core was less than 10,000 lines per sq. cm., so that saturation did not take place. When this machine was built and tested, it was found that four times the normal output of the current in the armature did not even distort the field, neither was the neutral point shifted, nor sparking produced. By normal output of current, I mean that current which would not excessively heat the armature on a long run, or a rating of 500 cir. mils per ampere in each conductor. I agree therefore, with the members who said that you may make the air gap as small as you please, if then, you only design the machine right. But most persons who hear this will say: "In what manner do you design the machine right?"

I have examined the complete data and dimensions, together with the corresponding performance of almost every sort of direct current commercial generator or motor, and have not found a set of conditions, where the relations that I have spoken of do not hold true. The point mentioned by Mr. Tesla as being one of such great importance as to change the whole result, is one not at all unknown to me and is one entirely covered in the paper. He said: "In a motor or generator, in which the polar faces and armature core are both smooth, we can reduce the air gap inde-

finitely, and the smaller it is, the better, and in many experiments I found that we can not reduce it far enough, disregarding mechanical difficulties." Now, one of the points I made is that when the poles are so proportioned and used, that saturation does not occur, the armature ampere turns have practically no effect on the total magnetization set up through the armature by the field. This is true no matter how small the air gap may be. If then we use a very small air gap, poles properly proportioned and without too small a space between them, there will be inactive coils in the normal diameter of commutation where the brushes may be placed, and the machine may be operated precisely as Mr. Tesla states. Such a machine, unless it be a very small one, will always have the magnetization reversed to a considerable extent under the pole corners *a. b.*, Fig. 1. In the larger machines this negative magnetization assumes larger proportions, and the magnetization under the strong pole corners becomes correspondingly large. As a matter of fact, in this line of practice, the manufacturers found it necessary to build up the faces of cast-iron poles with wrought-iron. In this way the strong pole corners are kept from saturation, a condition that was shown to be necessary if the armature current is not to seriously affect the total magnetization especially when small air gaps are used. This type of machinery, performs entirely in accordance with the relations treated of in this paper.

Another point was raised by Mr. Tesla, when he spoke of the effect of the six slots cut in the poles. He misunderstood me. The slots were entirely back of the pole surface. The pole surface was not interfered with by them, that is it was left smooth. All field exploration curves given in this paper are in absolute measure, and were obtained by careful observation from the machines under consideration. The dotted curve, therefore, in Fig. 11, shows the existence of an entirely uniform and unbroken field in the air gap. Therefore, it is seen from actual measurement that the slots did not influence the magnetic distribution in the air gap.

In conclusion I desire to say that the investigations leading to the results established have been prosecuted almost entirely in connection with many classes of well-known modern commercial machinery. For obvious reasons, it has been necessary in this paper to discuss only data and results obtained largely from machinery, that is not at the immediate present, part of the practice of this country.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, October 27th, 1891.

The sixtieth meeting of the American Institute of Electrical Engineers was held this date at No. 12 West Thirty-first street. The meeting was called to order by Vice-President Thomas D. Lockwood.

THE CHAIRMAN:—In the absence of our Secretary, Mr. Pope, Mr. Foster has kindly consented to act for the evening. The paper forming the principal subject for the evening will be one upon "Magnetic Reluctance," by Mr. A. E. Kennelly. Prior to reading the paper, it is possible we may have one or two items of business, Mr. Secretary, have we anything?

MR. H. A. FOSTER:—There is the report of the delegates to the International Electrical Congress at Frankfort.

MR. T. C. MARTIN:—I have in my hands the report of Mr. Carl Hering, the chairman of the delegation sent by the Institute to Frankfort I understand that the report has been presented to the Council this afternoon and read there. I think it will be quite in consonance with the wishes of the members if I move that this report be printed in the TRANSACTIONS of the Institute. It is of sufficient interest to deserve permanent record, and by so publishing it I think that that purpose will be achieved. Therefore I move that it be so printed.

[The motion was seconded.]

THE CHAIRMAN:—I presume that motion comprehends the acceptance of the report on the part of the Institute, and I will accordingly put it. It is moved and seconded that the report presented by Mr. Carl Hering of our delegation to the International Congress of Electricians at Frankfort, be accepted, placed upon file and printed in the proceedings of the Institute.

[The motion was carried.]

THE CHAIRMAN:—I understand, gentlemen, that this International Congress at Frankfort has been so good as to send us some publications of its proceedings, and I will ask Mr. Hering if he

can give us any more information upon that head and can suggest any disposition which should be made of those publications.

MR. CARL HERING:—There is no more information to be given except to say that the printed proceedings of the Congress will be presented to the Institute, and I think it would be well for the Institute to write a letter to the managers of the Congress, thanking them for sending us this material.

THE CHAIRMAN:—If there is no objection, the Chair will request the Secretary to write such a letter of thanks, or perhaps in view of the courtesy, it would be better to have a formal vote to that effect. Do you make that as a motion, Mr. Hering?

MR. HERING:—I make that as a motion.

[The motion was seconded.]

THE CHAIRMAN:—It is moved and seconded that the Secretary be instructed to write a letter to the management of the Congress at Frankfort, returning the thanks of the Institute for the copy of its proceedings, which we have received, or will receive.

[The motion was carried.]

The Secretary announced that the following Associate Members had been elected at the meeting of Council, October 27th.

Name.	Address.	Endorsed by
DE KHOTINSKY, CAPT. ACHILLES,	Electrician, Germania Electric Co., 505 Exchange Bldg., Boston, Mass.	H. Bottomley. Otis K. Stuart. Thos. D. Lockwood.
DUNSTON, ROBERT EDWARD,	President, The Connecticut Motor Co., Plantsville, Ct.	M. J. Wightman G Pfannkuche. Chas. L. Buckingham.
MIDDLETON, EDWARD T.	Instructor in Electricity and Physics, Rutgers College, New Brunswick, N. J.	T. C. Martin. F. A. Pattison. Ralph W Pope.
MONELL, JOSEPH T.	With F. B. Crocker, 236 W. 22d Street, New York City	F. B. Crocker. W. H. Freedman. Gano S. Dunn.
SMITH, T. CARPENTER,	Partner in firm of M. R. Muckle, Jr. & Co., 212 Drexel Building, Philadelphia, Pa.	Ralph W. Pope. H. A. Foster. T. C. Martin.
STRONG, FREDERICK G.	Electrician, Midland Electric Co., 1616 Seventeenth St., Denver, Colo.	E. Z. Burns. Wm. A. Anthony. John S Waring.

Total, 6.

THE CHAIRMAN:—The first of these names, Captain de Khotinsky, is of more than ordinary interest, since it was in a French patent awarded to that gentleman that one of the very earliest descriptions and drawings of a parallel circuit arrangement for direct distribution for incandescent lighting was disclosed. Such of us as have had patents rejected upon the French patent of Captain de Khotinski will, I am sure, welcome him with open arms when he makes his appearance here.

We shall now be happy to hear Mr. Kennelly read his paper upon "Magnetic Reluctance."

Mr. A. E. Kennelly read the following paper:

A paper read at the 60th Meeting of the American Institute of Electrical Engineers, New York, Oct. 27th, 1891. Vice-President Lockwood in in the Chair.

MAGNETIC RELUCTANCE.

BY A. E. KENNELLY.

The science of magnetism was a collection of facts concerning magnets, until Coulomb first brought to light a quantitative relation between a few of its phenomena, and thus entitled it to appear among the exact sciences. He determined by measurement that the forces of attraction or repulsion between the poles of long thin bar magnets were proportional to the strengths of those poles and inversely to the square of their intervening distance.

In one respect, it has since been shown that the discovery was unfortunate, for it served to depress rather than to stimulate further inquiry into the laws of quantitative magnetic relationships. The application of Coulomb's law soon brought into use a conception of magnetism, suggested perhaps by the analogy the law bears to that of gravitation force. This was the hypothesis of a layer of fluid or imponderable matter resident on the surfaces of magnetic bodies and endowed with attractive and repellant forces on all portions of such fluid, in exact similarity to the two fluid theory of electricity. Each element of surface magnetism would exert according to Coulomb's law a definite force upon every other element of its own, or of other magnet surfaces, and when the distribution of the magnetic matter or fictive layer was known, the total forces active between the magnets forming the system could be determined by the summation of all the elementary actions. This was the polar conception and mathematical theory of magnetism. It was not only artificial, it was also misleading. It assumed that definite action could be exerted at a distance, ignoring the action of the intervening medium. Never-

theless, a slight modification of the polar theory rendered it capable of expressing a mathematical theory of magnetism with apparent success, and exhibits in this respect, like the theory of gravitation, the remarkable construction of purely artificial frameworks of thought, void of all attempt at reality, yet capable of affording useful applications and exact quantitative results, while beneath their foundations, the real and active natural forces still lie in undiscovered concealment.

It was soon apparent that magnetism considered as a fluid could not be confined to the surface of bodies, since it was only necessary to break a bar magnet asunder in order that new poles and new magnetic fluid should be exhibited. The amendment to the original theory was then framed that a condition of molecular magnetization extended, veinlike, throughout the substance of the magnet. The termination of each vein at the surface exposed a definite quantity of polarized magnetic matter, while within the veins, the polarity was neutralized by the successive layers of opposite molecular poles. This was a great stride beyond the original theory, for it ascribed magnetism not alone to a fictive superficial layer, but to the combined effects of all the molecules in the magnetized body, whose substance, no longer a mere frame for supporting the layer, became invested throughout with a definite condition.

The amendment in time became classical, and subverted the original conception, for the polar theory only recognized superficial magnetism, while the vein theory did not make any such reservation. It was only necessary to assume that the veins varied in richness at different parts of the magnet, or that they might be subjected to faults and partial discontinuities, to introduce unneutralized polarity within the mass which might be capable of exerting external influence, a condition neither the simplest nor most usual, but which the experiment of superposed magnetizations in steel has rendered familiar. This irregularity of vein structure would add a volume distribution of magnetic fluid through the interior of the magnet to the surface distribution on the outside.

On the vein theory, the intensity of magnetization at any point of the magnet would be defined as the amount of magnetic matter cut through in the veins per unit area by a plane section perpendicular to their direction, at the point in question.

A unit magnetic pole situated in free space, at the end of a

very long thin bar magnet would by definition repel with unit force a similar pole one centimetre distant, that is resident on the surface of a sphere one centimetre radius, of which it occupied the centre. The surface area of this sphere would be 12.57 (4π) sq. cms., and over all this surface the pole would thus establish a magnetic field of unit strength and exert a magnetic stress equal to unity. Each square centimetre of the surface bounds an area of stress which emanates from the pole, and consequently the latter must emit 4π units of stress flux. In the language used by Faraday it would throw off 4π lines of force. This stress flux has somewhat unfortunately received from Maxwell the name of induction, and the induction through the surface surrounding the pole would be 4π units.

The interest connected with the nature of this stress demands a moment's pause at this point. While its real character is still as great an enigma as the real character of magnetism itself, some of its laws are well understood. There exists along the direction of the stress a tension accompanied by a general pressure at right angles, just as in the case of stimulated muscular fibre which tends to shorten and to expand laterally, or as it is generally expressed, the lines of force tend to contract while repelling one another. Since the time of discovery that the plane of undulation in polarized light waves, twists in passing through certain magnetized substances it has been supposed that the stress might be of a rotatory character. Like an incompressible fluid it can neither accumulate nor vanish locally, and as much stress flux must leave any region as enters it, if no magnetic pole exist within the confines. Its intensity is very remarkable, for it is far in excess of any stress that we can exert without the aid of cohesion in matter. This may be judged by comparing the quantities of energy that can be stored in a given volume of space—say air space. It is of course easy even without the assistance of iron to magnetize a cubic centimetre of air to a flux density of 5019 C. G. S. lines when it will possess an energy stored up in magnetic stress of one megalerg (1,000,000 ergs or 0.0737 foot-pound) while if this magnetization be alternated 227 times per second the average rate of absorption and discharge would be 45.5 watts, and one cubic inch of this air would stow and release energy with the average activity of one standard British horse. Under electrostatic stress, air not in the immediate neighborhood of a conductor appears to break down when the E. M. F. per linear centi-

metre exceeds 32,000 volts or 110 A. E. S. U., so that the maximum electrostatic energy that one cubic centimeter will hold without rupture is about 500 ergs or two thousand times less than that resident in air stressed magnetically to 5,000 lines, while a cubic centimetre of air near the earth's surface illumined by bright sunlight does not hold more radiant energy than about 50 microergs.

An alternating current transformer is a device in which the secondary coil absorbs the energy out of air and iron, pervaded by the stress flux from the excited primary coil. Unfortunately while magnetically stressed air yields so far as we know all its stress energy back into either the primary or secondary circuit, stressed iron fails to do so entirely, and absorbs energy at every cycle, a process known as absorption by hysteresis The iron directly converts the magnetic stress energy into heat energy, just as a spring absorbs some of the energy imparted to it during every cycle of compression and recoil.

These 4π units of stress flux are not confined to a spherical surface of one centimetre radius but would be found by summation over any enveloping surface containing a single unit pole, just as the delivery of material liquid must be the same in a steady stream at any section of its course independent of the channel area there existing; for at ten times the distance, the spherical area would be 100 times greater and the stress by Coulomb's law 100 times less, leaving the total area of total stress unchanged, and it follows in fact that the stress flux over any boundary is always 4π times the strength of pole enclosed, no matter whether that pole be alone in space, or be the sum of any number of magnetic systems, in molecules or in masses.

On the vein theory however, the strength of a pole is the quantity of magnetic matter on its surface, assuming that the veins meet it perpendicularly, or it is the aggregate surface intensity of magnetization, so that the stress flux through each unit of polar surface is 4π times the intensity of magnetization. This is true not only for the polar surface, but also for the interior of the magnet and every unit vein of magnetic polarized matter carries associated with it a stress flux of 4π.

The magnet may itself be situated in a field of force so that its substance may be pervaded by a stress flux independent of that in its veins. It may and generally does happen that part of this separate stress flux issues from its own poles, in which case it will enter in the opposite direction to the vein or internal flux

and will tend to reverse the molecular polarity of the vein structure, but whenever such additional flux is present, the resultant total flux at each point will be the geometrical resultant of that in the veins, compounded with the external flux by the parallelogram of forces. In the important case of a long bar of homogeneous soft iron submitted to the influence of a uniform magnetic field acting in the direction of its length there will be a uniform induced molecular polarization or magnetization, set up along the bar. Suppose for instance a bar of iron five square centimetres in uniform cross-section pointed in the direction of the dipping needle near Paris. The earth's field there being 0.46 unit, or its stress flux 0.46 units over each normal square centimetre, that stress would be active in setting up polarity in the molecules of the iron along the bar. The degree of magnetization would be also influenced by its own resulting polar stress unless this effect could be rendered insensible by sufficiently extending the length and thus carrying the poles out of the field. With a very long bar magnet the magnetization would be everywhere about 40 times this strength of field, or numerically 18.4 units, and on the vein theory there would therefore be 18.4 units of magnetic matter exposed on the surface of each square centimetre in a slice made across the bar at any point or 92 units over the whole section. The polar surfaces would also be covered by 92 units of positive and negative matter respectively. Associated with each unit vein of polarity however would be 4π units of stress flux giving a total flux of 1156 units in the bar which would emerge from the polar surfaces and unite in surrounding space. The whole induced system would however be immersed in the earth's field that evoked it, and which would be superposed upon it throughout. There would thus be 1156+2.3 or 1158.3 units of flux in ordinary phaseology 1158.3 lines of induction through the bar, a density of 231.6 c. g. s. units per square centimetre. The law that the whole flux is the field flux compounded with the induced flux of 4π times the magnetization, which holds uniformly at all points throughout the bar in this simple case is true at any individual point in the most complex case, however variable the field or magnetization may be.

The fact that the stress flux has the same total value over any envelope surrounding a magnetic pole, however widely the stress may be diffused and rarefied, that it follows curves in space whose existence can be rendered evident by iron filings and curves that

form closed loops through the substance of the magnet itself gradually suggested the notion of a magnetic circuit in which the circulation was neither a fluid nor an energy-exchanging condition like a current but a stress.

This conception once firmly established proved of great advantage. Not only has the dynamo been greatly aided in development by the applications of this theory but the inter-relationship between magnetism and electricity has been brought into clearer recognition in consequence. From this point of view the ideas and analogies of the galvanic circuit became paramount and eclipsed the original notions of magnetic matter and magnetization. All that was essential on this hypothesis was a magneto-motive force in a circuit having conductivity, and a flux or magnetic current resulted. The magneto-motive force in permanent magnets was the result of a definite molecular condition in the iron, while in the neighborhood of an electric current it was always active. According to Ampère's theory that magnets had molecules in which electric currents ever circulated, the two sources of magneto-motive force were united.

Some contention took place between the claims of the vein theory and the circuit theory for the interpretation of magnetic phenomena, and the question as to their relative merits is yet often raised. It seems however early to decide upon the acceptance of any theory while the ultimate origin and nature of magnetism remains unexplained, and it is better to regard both theories as working hypotheses to account for the effects of magnetic laws, equally capable of yielding correct results and therefore closely associated, while the preference between them will depend upon the nature of the problem to be attacked. The circuit theory is the simpler for general purposes of theory and particularly for dealing with the phenomena of electromagnetism. The vein theory on the other hand while very artificial is often more convenient in dealing with the magnetic behavior of the three metals iron, cobalt, and nickel, and it is the natural accompaniment of Ewing's modern theory of magnetization which at present appears to best represent the facts.

On the circuit theory the magnetic conductivity of all substances is very nearly the same as that of an air-pump vacuum, and constant, notably accepting the three magnetic metals whose conductivity is enormously greater, but variable with the conditions of the circuit.

The existence of magnetic conductivity known as "permeability" necessarily involved the inverse quality of magnetic resistance.[1] Some debate has taken place as to the validity of this term or its application, for the reason that the resistance which enters into practical magnetic circuits is generally far more variable than that found in the metallic circuit of electricity. It is now generally admitted however that the term magnetic resistance is fairly applicable in virtue of analogy if not of exactitude. The term magnetic reluctance then suggested by Heaviside, has however the advantage that "reluctance" as one word is less cumbrous than "magnetic resistance," and for that reason only, it is advantageous to retain it. Similarly the reluctance per unit volume or the magnetic resistance of one cubic centimetre has been called "reluctivity" and is perfectly definite if not quite euphonious under that title. It is this quantity which will occupy our further attention.

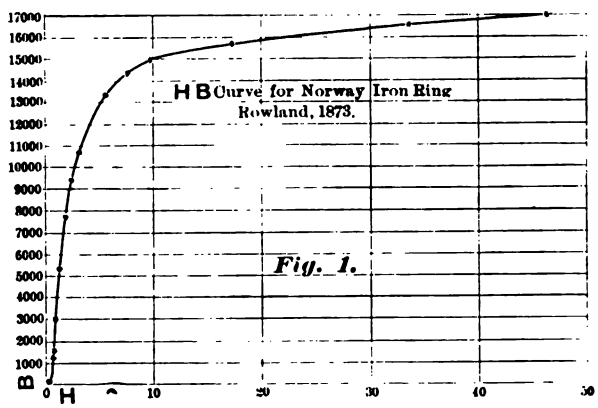
This magnetic circuit differs from the galvanic in one important particular. In the latter we most frequently meet with a conductor carrying the flux or current through an insulating medium such as air in which no current—certainly no measurable current—passes. In the magnetic circuit this condition of things while approximated to, and often for practical purposes assumed in dynamos or electro-magnets is only realized in one actual instance—that of a closed Faraday ring uniformly wrapped with wire carrying a current In every other real case the flux is diffused more or less through the surrounding substances and does not follow a simple passage. Magnetic flux in fact is not to be enchanneled because there is no known magnetic insulator, and this fetters by complication the quantitative grasp of the subject.

The law of the magnetic circuit is like Ohm's law that the flux is equal to the magneto-motive force divided by the reluctance. That is regarding the circuit as a whole. For any one point of the circuit the law is, that the flux density is equal to the drop in magneto-motive force per centimetre, there existing, divided by the reluctivity—corresponding exactly to the case of Ohm's law at any point in an electric circuit. This "drop" is called the magnetizing force, denoted by H, and is the strength of the magnetizing field at the point. Owing however to the great variation that exists in the reluctivity of iron which enters into nearly

1. Bosanquet. *Phil. Mag.* Vol. xxv, 1888, p. 419.

every practical magnetic circuit, the law is of much less service than Ohm's electric law. The reluctivity depends not only on the magnetizing force but also on the degree of purity of the iron, and iron possessing 12 per cent. of manganese is almost non magnetic.[1]

A number of attempts have been made by different observers to establish a definite relationship between the flux density and the magnetizing force or the permeability in order to so amend the magnetic Ohm's law as to give serviceable results for even pure iron. The results obtained, excepting the formulæ of Frölich and Lamont, have generally been regarded as unsatisfactory. They have usually been expressed graphically as curves in one of three types:—



1. H—B or curves of magnetizing force and flux density.

2. H—μ or curves connecting permeability and magnetizing force.

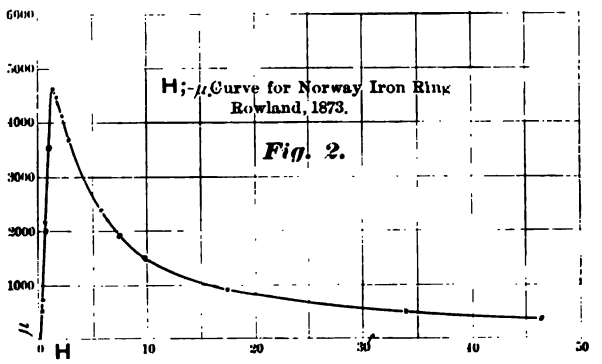
3. B—μ or curves connecting flux density and permeability.

It is the object of this paper to show that there is a fourth relation that has not received the attention it seems to deserve—namely the curves connecting the reluctance with the magnetizing force.

As an example of the comparative forms of these well-known curves, types 1, 2 and 3 are represented in Figs. 1, 2 and 3 for a sample of Norway iron whose results are among the first on record, having been published in 1873.[2] The curve connecting the

1. Hopkinson. *Phil. Trans.* 1885, part ii, p. 462.
2. Rowland. *Phil. Mag.* 1873, p. 153, table v.

reluctivity with magnetizing force is shown in Fig. 4 and exhibits a greater degree of simplicity than any of the others. Curve 4 consists in fact of two lines that are very nearly straight, united by a rounded elbow; and these outlines express the fact that the reluctance of this sample of iron was very nearly a linear function of the magnetizing stress brought to bear upon it. The ordinates for this curve are given in thousandths of an absolute reluctance unit, a quantity we may call provisionally a milli-unit abbreviated *MU*. The reluctance of 1,000 *MU*. in series would be that of one cubic centimetre of air. Reluctivity curves plotted from various sources all of which are duly given in the Appendix appear in Fig. 6 for various samples of iron and steel. The general type consists of two lines nearly straight, connected by an elbow which



is sharper and more defined as the iron is softer and more nearly pure. With hard steel the change from the descending to the ascending line is very gradual. The reluctivity of iron appears from these curves to commence at a certain definite and moderately large value which we may denominate its initial reluctivity, descends very rapidly and nearly on a straight line to what may be called the critical reluctivity at the critical magnetizing force, turns abruptly, and then advances along a nearly straight line. Before the turning point in Fig. 4 the reluctivity may be expressed approximately by the equation $\rho=2.9-3.5H$ while beyond the bend the equation becomes $\rho=0.1+0.058H$. The flux density existing in this sample of Norway iron can be calculated by the Ohm's law formula for any value of H, when the corresponding value of the reluctivity is taken from one or other of these two equations and the resulting curve of $B-H$ will practically coin-

cide with that in Fig. 1 except in the immediate neighborhood of the critical value of H This relation also exhibits the foundation that exists for the Frölich formula which has long been known to be practically valuable, and even very accurate in application to dynamo-magnetic circuits, although both the method by which it was arrived at and the light in which it has since been regarded, appear to have been empirical rather than fundamental.[1]

Fig. 5 shows that the minimum reluctivity for soft iron is about 0.15 *MU.* while its initial reluctivity is about 3.0 *MU.* The critical value of H. is also in the neighborhood of 2. If any critical H can be said to exist for glass hard pianoforte wire it would be in the neighborhood of 35 units, and every description of hard or impure iron met with in practice, not containing much manganese, appears to have its critical H be-

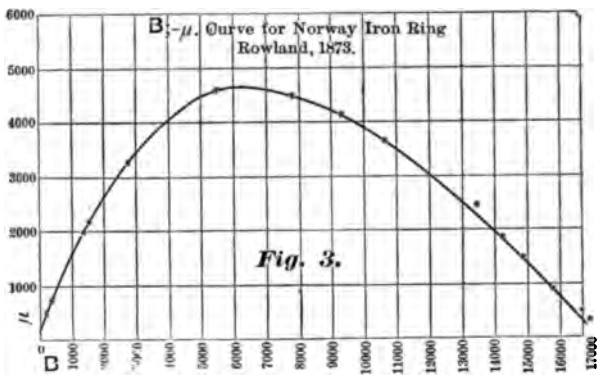


Fig. 3.

tween these limits. These reluctivity curves also illustrate perhaps more clearly than any others the leading features of Ewing's theory of magnetization. There is first the initial stage of diminishing reluctivity during which the imaginary molecular magnets are being deflected from their original configurations. then a short critical range of magnetizing force in which nearly all the original groupings are disrupted at the point of lowest reluctance, and finally a steady increase of reluctance as the molecules are gradually forced by increasing magnetic stress into parallelism.

This bilinear characteristic curve of reluctivity is not confined

1. Frölich. *Electrotechnische Zeitschrift.* pp. 90, 139, 1881, p. 73, 1882.
Silvanus Thompson. "Dynamo Electric Machinery." p. 305.
"Lecture on the Electro-magnet." I. p. 74.
Ayrton & Perry. *Phil. Mag.* 1888, vol. xxv, p. 506.

only to iron. Fig. 6 shows the same general outlines in the case of nickel taken from the observations of Rowland and Ewing. Here the initial reluctivities are higher, and the descent to the critical points much sharper than in iron, while the ascending lines are also steep but nearly straight.

Taking the equation $\rho=a+bH$ in the ascending curve the first term a only disappears in one known instance—that of nickel under a mechanical stress of 19.8 kilograms per square millimeter or 28,160 lbs. per square inch—as seen in Fig. 7 taken from Ewing's results. The lowest ascending curve prolonged downwards

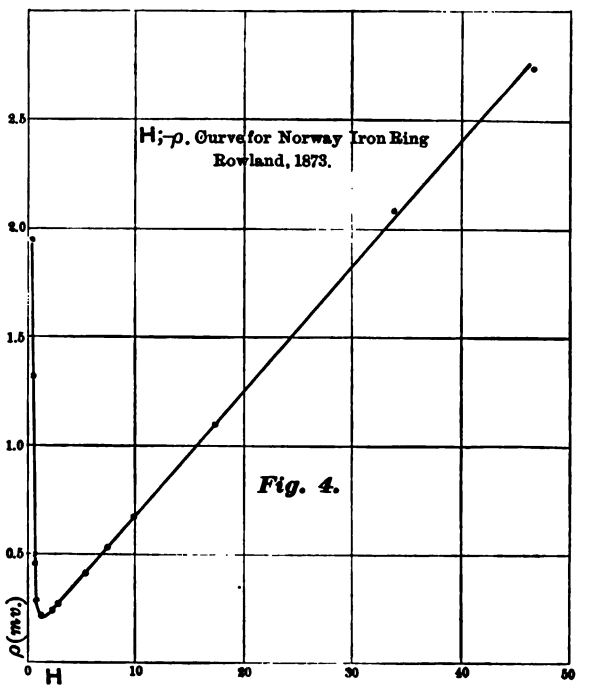


Fig. 4.

almost meets the origin. Consequently the flux equation for such nickel would be

$$B=\frac{H}{bH}=\frac{1}{b}=\frac{1}{0.000177}=5650$$

and this flux is practically constant for any magnetizing force within further considerable limits, representing early saturation. The influence of compression in diminishing the reluctance of nickel is clearly shown in Fig. 7. The critical reluctance is gradually diminished by the stress applied from 14.5 *MU.* at *H.* 30 to 2.6 *MU.* at *H* 12.

Fig. 8 gives the corresponding curves for cobalt. The same features are again observable. Only in one case, that of cast cobalt at 230° C, is there any considerable deviation from a straight line in either rise or descent from the critical segment of the curve, and this represents a series observed by Rowland under some experimental difficulties and alluded to as somewhat doubtful.

We have now examined the reluctance of the three magnetic

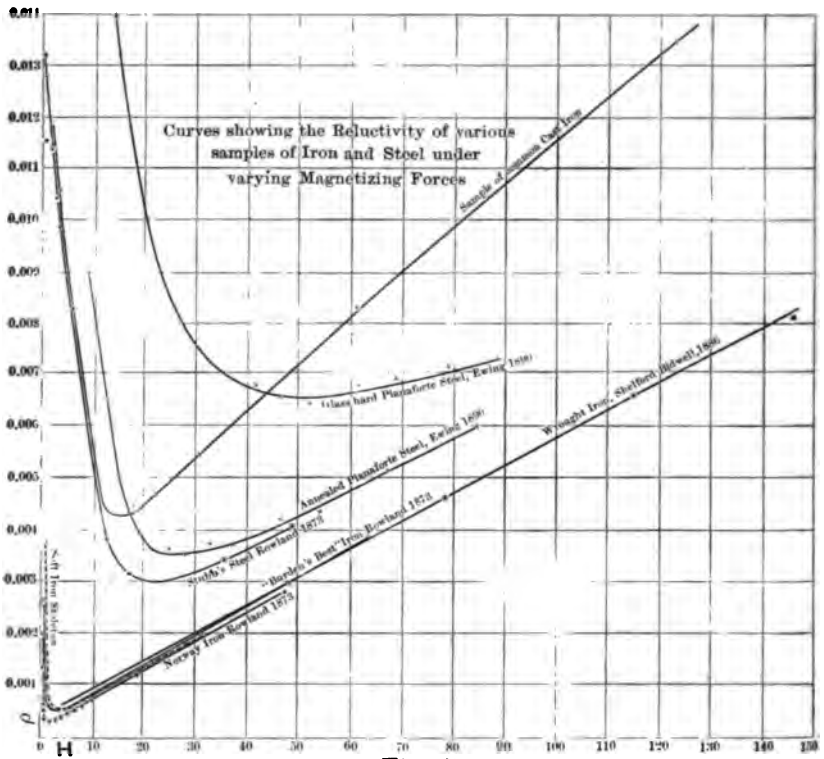
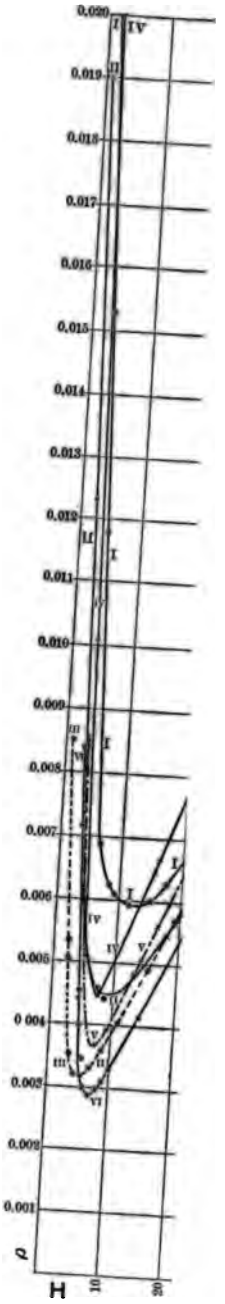


Fig. 5.

metals collected from all the best known experimental observations. In addition to these it appears that every reliable series of tabulated results for iron, pure, impure, cast or forged, exhibits the same types of bilinear reluctance referred to H between 0 and 150, although in very hard or impure specimens there is no clear indication of a critical point. This assumes however that the observations are not vitiated by residual magnetism. For example the curve obtained by plotting the reluctivities of cast-iron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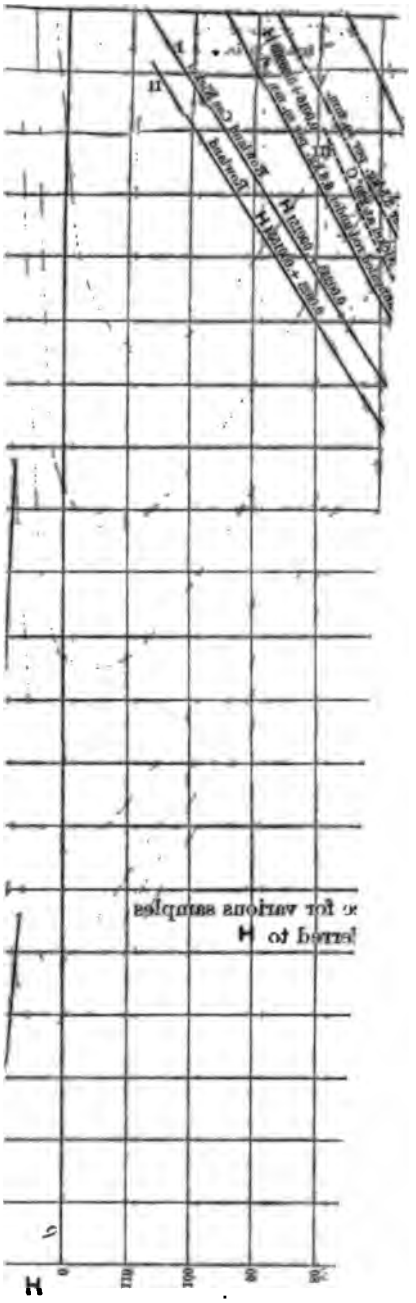
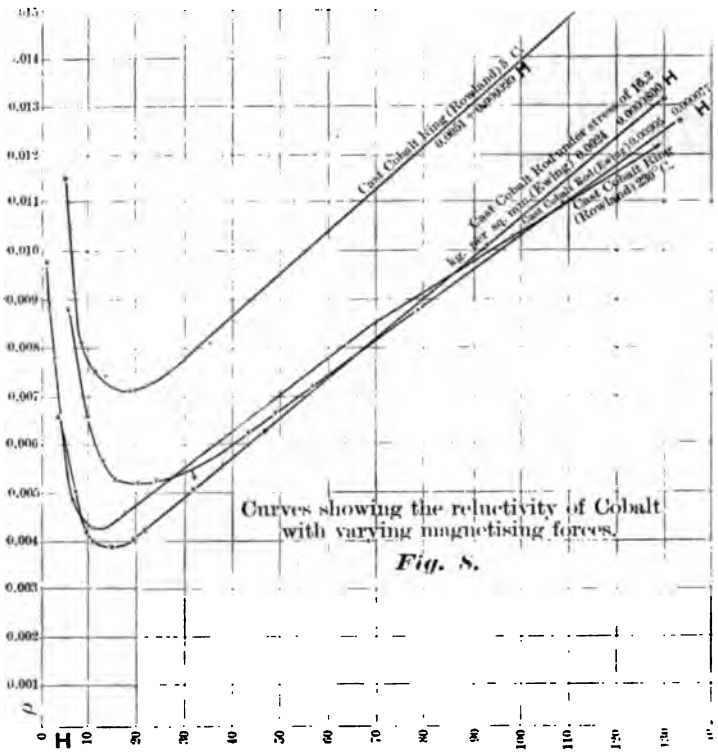

H
0
10
100
80
90

Ewing's Experiments on Effect of Stress with a Bar of Hard Steel
IV
III
VI
V
IX
XII
XIII

Fig. 17.

oted by Silvanus Thompson from Hopkinson's observa- differ considerably from this type, but the difference ap- ₃ to be accounted for by taking the mean between ascending descending values. The ballistic method of measurement anchor rings, in which the progress of the magnetizing force s place by extending reversals, gives reluctances that appear e as nearly linear as those which are obtained by a single in-



Curves showing the reluctivity of Cobalt with varying magnetising forces.

Fig. 8.

-creasing amplitude of magnetization, the influence of hysteresis being apparently nearly uniform in ratio.

An examination of reluctivity curves naturally suggests the question as to whether there is really a strict linear relationship between H and ρ. In other words whether the divergences of the observation curves from geometrical straight lines can be fairly ascribed to errors of observation allowing for the influences of residual magnetism.

First confining the inquiry to the ascending reluctivity line—

1. Silvanus Thompson. "Dynamo Electric Machinery," p. 302.

that which is geometrically consequent upon Frölich's formula and also under special interpretation with Lamont's formula.—the agreement of the plotted observations with a straight line between the neighborhood of the critical H and $H=150$ is often so good as to intimate the existence of a definite linear relationship. It is generally to be found, however, that beyond 150 c.g.s. units of H, the line bends downward until all hope of

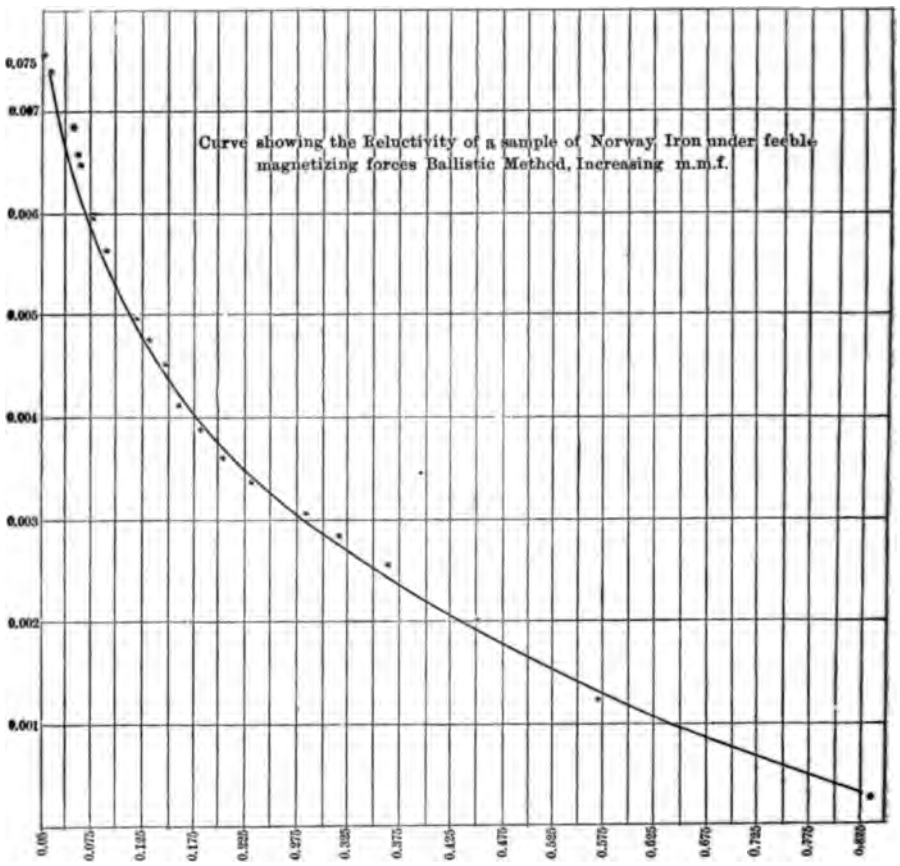


Fig. 9.

rectilinearity is lost. That is on the simple circuit theory of reluctance. If, however, we introduce an amendment into the definition of reluctance, borrowed from the vein or polar theory, the rectilinearity appears to be nearly sustained for a much greater distance. Experimental observations of the reluctances in circuits of the magnetic metals under powerful magneto-

motive forces are yet very scanty, but judging from the results of Ewing and Low[1] the reluctivity of wrought and cast-iron on the amended definition appears to be a linear or at least nearly linear function of the force as far as H 25,000 and H 11,000 respectively, the limits of the quoted measurements.

The graphs of these measurements are given in Fig. 10. The observations run from H 3630 to 11,200 for wrought-iron with an isolated observation at H 24,500 in a separate instance, the similar series for cast-iron running from 3,900 to 10,610 units of H. The linear relationship is very fairly maintained and the lines prolonged downwards nearly strike the origin. It is to be observed that at the limiting observation for wrought-iron its reluctivity is nearly 20 per cent. greater than that of air or of the air pump vacuum.

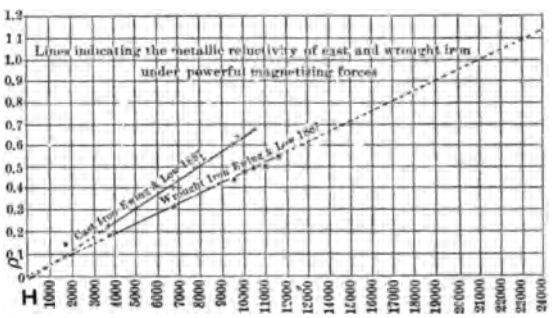


Fig. 10.

According to the simple circuit theory, the reluctance is of course the ratio of the magneto-motive force to the flux, and the reluctivity this quantity locally reduced to the unit of volume. On the vein theory, however, which as we have seen distinguishes the vein flux from the magnetizing flux superposed thereon, the conductivity of a mass of iron is the conductivity of the iron itself added to that of the space it occupies, and consequently applying the vein theory to the magnetic circuit we have the apparent reluctance of the iron mass as the joint reluctance of two paths in multiple arc, one through the iron itself and what might be called its metallic reluctance, the other through the re luctance of the space occupied by the iron, and the removal of the iron would leave this latter unaltered. The difference between the apparent and metallic reluctance is inappreciable

1. "*The Electrician*," London, 1890, July 25 and Aug. 1.

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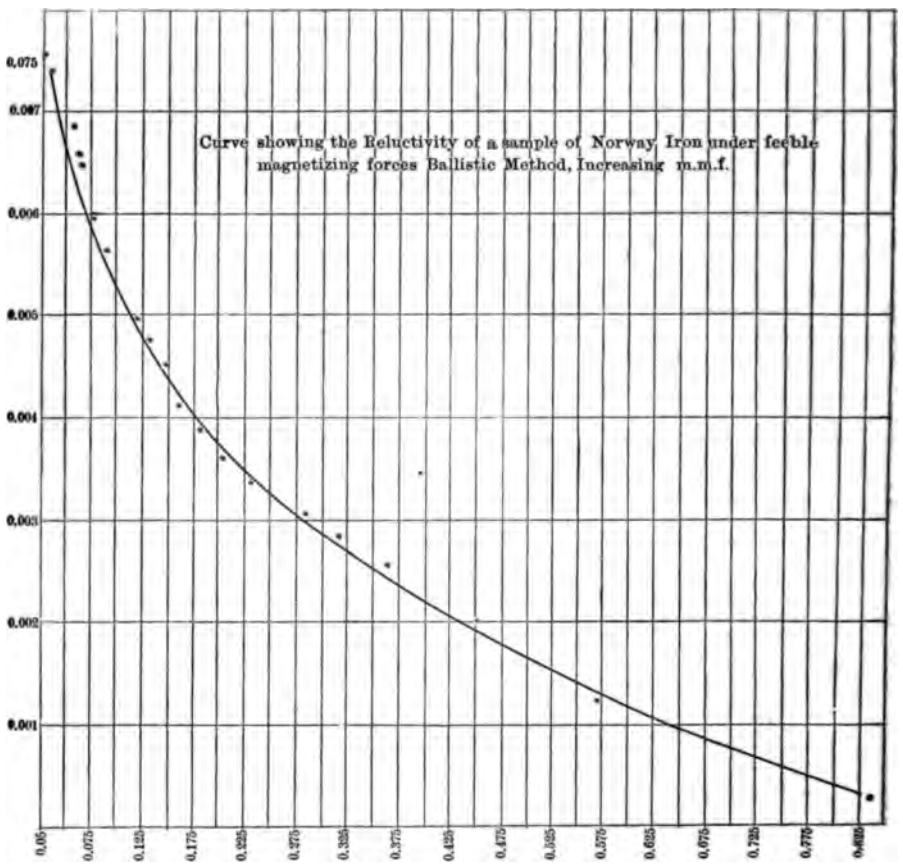

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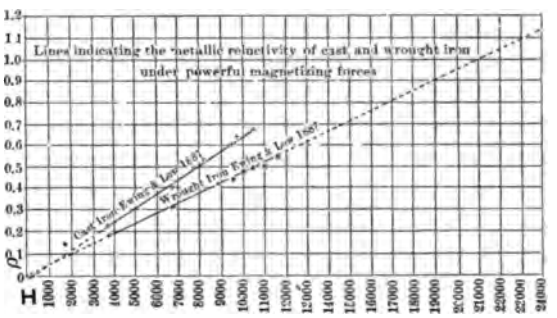

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1. "*The Electrician*," London, 1890, July 25 and Aug. 1.

while the latter remains small, that is generally speaking when H is below 150, a limit rarely exceeded, and consequently the question does not present itself under usual practical conditions, but for large values of H, the difference is considerable and the metallic reluctance approaches the linear relationship with H while the apparent reluctance deviates considerably from it.

These assumptions from the vein theory, while they may be convenient are somewhat artificial for they postulate that the space reluctance of a given volume of air is not altered when the volume is occupied by iron. This may not be impossible but it is difficult to imagine any reluctance mechanism of ether that would remain undisturbed by the introduction of a massive substance. On the other hand while the vein theory imposes this principle not touched upon by the circuital hypothesis, it explains very satisfactorily the fact now apparently beyond dispute, that while iron can be saturated, there is no limit yet attained to the flux density that can be made to pass through it. Ewing's results give no limit at the observed flux density of 45,350 C.G.S. nearly three times the flux density at which iron is commonly worked in practice, while at the same time they indicate a limiting value of magnetization long before that density is reached. Following the vein theory, the polarization of the iron is then complete, and the intensity of magnetization or magnetic matter per unit cross-section of veins finds its maximum so that while the total flux can go on increasing indefinitely, it can only do so by adding to the permeating field flux, the vein flux having reached its full limit. It is not impossible to represent the observed condition of affairs by the simple circuit theory, but the mental picture is not so clear. It would be possible for instance to imagine that the molecules of all substances transmitted the stress flux with the same or almost the same facility as the ether surrounding them, but that in the magnetic metals they exalted the stress in transmission. Maxwell supposed that the iron molecules were so constructed that they could take part in the ether spin that might constitute the stress, and if so by adding to it their momentum of revolution they could augment its value. There would be then perhaps at a certain stress, a speed of revolution which the iron molecules would not exceed, and their reinforcement would be at a maximum, while for stresses enormously greater than this, the molecular augmentation would be lost in comparison with the strength of field and the

iron molecules would in the aggregate behave almost like the motionless transmitters of other substances. The consequences of this conception seem more complex even if more nearly true.

Turning now to the descending curve of reluctivity between the initial and critical values, closer examination will show that here at least the linear relation is only an apparent one. The descent is so steep that on the scale of projection it appears nearly straight, but when magnified it has a distinct curvature. Rayleigh[1] and others have shown that for very small degrees of H the permeability commences with a definite steady value and this being the case it would be impossible for the reluctivity—the reciprocal of that permeability—to be linear towards H. Series of observations covering with sufficient detail the range of H from zero to unity are apparently few, and Fig. 9 gives the plotted values of the reluctivity on an enlarged scale observed by the writer for a sample ring of Norway iron. The descending line has a marked curvature approximately logarithmic, that would be almost inappreciable however on the scale of the other reluctivity diagrams.

Even however if we admit that there exists a linear relationship between H and ρ beyond the critical point, that is to say if the experimental results justify the belief that Frölich's formula is not merely an empirical one, we are scarcely entitled to attribute to this relationship an intrinsic physical signification. It may enable us to grasp the salient features of the magnetic circuit by the re-establishment of Ohm's law, but the relationship is more likely to be the consequence of a more remote fundamental agency than to be significant of any physical condition resident in metallic reluctivity itself. This is for the reason that the increase of flux under M. M. F. in a magnetic circuit due to the presence of iron is more probably owing to an assisting M. M. F. set up in that iron under stress than to any change in the latter's reluctance, and the removal of the initial source of M. M. F. from the circuit still leaves some M. M. F. active as residual magnetism.

The case is similar to that of an electric circuit containing a polarizing electrolyte. It might be more simple to ignore the counter E. M. F. of polarization and to regard the resulting diminution of current as the result of an extra resistance whose value might be tabulated or possibly even reduced to simple laws. So again in alternate current circuits it is often more convenient to

1. *Phil. Mag.*, March, 1887.

speak of a resistance coil with inductance, as possessing an impedance greater than its ohmic resistance, and to determine the flow of current on the basis of impedance calculated to the original E. M. F. whereas the direct and fundamental method would be to keep the resistance at its true value and determine the current by allowing for the counter E. M. F. of inductance in quantity and in time.

Retaining then the latter analogy, we may say that flow in an electric circuit is subject to Ohm's law simply; when the E. M. F. is constant, but when the effective E. M. F. varies periodically, the resistance has to be increased by a definite amount depending on the inductance and the manner of fluctuation in order to obtain the correct effective current under those conditions. Similarly that the flux in a simple magnetic circuit of air or vacuum is subject to Ohm's law, but that if the circuit includes a magnetic metal, the reluctance in the formula has to be changed in a definite way depending on the quality of the metal and on the magnetizing force in order to obtain the corresponding true flux density. The change in reluctance is the ideal connection of a shunt metallic circuit in parallel with the air circuit, and the reluctivity through the metal is nearly $a^1 - b\,H$ for values of H below the critical, and $a + bH$ for values beyond, with a little uncertainty in the vicinity of the critical point itself.

IRON.

Observations of Dr. Stoletow on a ring of soft iron published in the *Philosophical Magazine* for 1873. No. 45, p. 86.

H	k	ρ
0.4302	21.54	0.003695
0.5497	23.78	0.003346
0.7017	26 44	0.003010
0.9220	40.95	0.001943
1.053	51.10	0.001557
1.151	59.76	0.001331
1.260	68.70	0.001159
1.367	76.53	0.001040
1.494	84 53	0.0009414
1 560	104.48	0.0007618
1.647	113.5	0.0007012
2.321	157.0	0.0005069
3.212	174.2	0.0004569
3.562	172.3	0.0004619
3.814	170.7	0.0004663
4.038	168.9	0.0004711
5.247	161.6	0.0004924
6.789	141 7	0.0005616
7.183	136.2	0.0005842
7.555	132.1	0.0006024
8.326	120.	0.000662
9 14	112.2	0.0007093
10.035	108.1	0.0007363
10.503	104.2	0.0007638
11.118	97.12	0.0008197
11.96	93.97	0.0008467
13.26	87.70	0.0009072
14.01	82.08	0.0009693
15.60	75.43	0.001055
17.93	66 87	0.001190
19.57	61.93	0.001285
20.59	59.22	0.001344
21.70	56.47	0.001409
22.80	53.92	0.001476
23.58	52.88	0.001505
25.22	49.68	0.001602
27.27	47.29	0.001683
28.82	44.04	0.001807
29.61	43.65	0.001823
30 73	42.13	0.001889

Columns I. and II. are taken from the record, Column III. is calculated from Column II.

Observations by Professor Rowland on a ring of Norway Iron. Published in the *Philosophical Magazine* for 1873, No. 46, page 153, Table V.

H	μ	ρ	β
0.1689	512	1.953×10^{-3}	86.5
0.3359	759	1.318	255
0.6485	2005	0 499	1300
0.7002	2187	0.457	1531
0.845	3567	0.280	3014
1.169	4602	0.217	5380
1.711	4545	0.220	7770
2.248	4140	0.242	9300
2.877	3680	0.272	10590
5.52	2429	0.412	13410
7.426	1917	0.522	14240
9.894	1507	0.664	14910
17.30	906	1.104	15680
33.72	480	2.083	16580
46.32	364	2.747	16850

BURDEN'S BEST IRON.

H	μ	ρ
0.1829	391	2.56×10^{-3}
0.6912	869	1.15
0.8564	1129	0.886
1.2705	1936	0.516
1.406	2078	0.481
1.451	2124	0.471
2.039	2433	0.411
2.219	2470	0.405
2.339	2472	0.4045
2.717	2448	0.4085
3.156	2367	0.4224
4.050	2208	0.453
5.369	1899	0.5265
9.474	1448	0.691
10.225	1269	0.788
11.99	1137	0.880
17.64	824	1.213
34.15	462	2.165
45.99	354	2.826
64.32	258	3.876

Norway Iron.

Results obtained with a welded ring of Norway iron under weak magnetising forces.

H	B	ρ	μ
0.00727	3.767	7.24×10^{-3}	138.
0.0575	8.85	6.502	154.
0.075	12.9	5.82	172.
0.092	16.9	5.45	183.5
0.117	24.6	4.76	210.
0.136	29.6	4.58	218.
0.149	33.8	4.40	227.
0.161	38.9	4.15	241.
0.176	45.1	3.91	255.8
0.199	54.6	3.65	274.
0.229	67.0	3.42	292.4
0.273	83.2	3.28	304.9
0.3136	108.3	2.90	344.8
0.3736	146.1	2.557	391.1
0.451	212.	2.13	469.5
0.572	421.6	1.357	737.
0.827	1725.	0.480	2083.
1.295	3647.	0.355	2817.
3.093	7838.	0.3945	2535.

STEEL.

Observations by Professor Ewing on a wire of annealed pianoforte steel. Published in the *Electrician*, London, April 25, 1890.

H	B	ρ
9.	1000	9.0×10^{-3}
13.	2000	6.5
15.	3000	5.0
17.5	4000	4.4
19.5	5000	3.9
21.5	6000	3.58
25.	7000	3.57
28.	8000	3.5
33.	9000	3.66
37.	10000	3.7
45.	11000	4.1
52.	12000	4.33
66.	13000	5.1
84.	14000	6.0

Glass hard steel pianoforte wire same date of publication.

H	B	ρ
14	1000	14. x 10^{-3}
20	2000	10.
25	3000	8.33
30	4000	7.5
35	5000	7.0
41	6000	6.8
46	7000	6.57
52	8000	6.5
61	9000	6.8
69	10000	6.9
79	11000	7.2
88	12000	7.3

STUB'S STEEL WIRE.

Observations by Professor Rowland, published in the *Philosophical Magazine* for 1873.

H	B	ρ
0.210	15.9	13.17 x 10^{-3}
0.784	67.8	11.56
1.362	119.7	11.38
2.567	244.8	10.48
3.41	344.6	9.90
5.30	627.8	8.45
12.89	3370.	3.823
17.15	5430.	3.158
34.32	7777.	3.126
34.47	10.080.	3.418
41.96	11.130.	3.768
44.71	11.500.	3.893
48.56	11.940.	4.065

NICKEL.

Cast Nickel Normal.

Observations by Professor Rowland, as published in the *Philosophical Magazine*, No. 46, 1873, page 153, Table VI.

M	H	λ	ρ
1.433	1.801	595	0.0211
2.904	3.650	819	0.01586
3.527	4.433	1070	0.01175
5.555	6.937	1815	0.006925
6.783	8.524	2017	0.006231
7.401	9.301	2063	0.00609
9.273	11.65	2114	0.005945
11.78	14.80	2098	0.005999
13.43	16.88	2018	0.006227
16.53	20.77	1878	0.006689
21.02	26.42	1663	0.007559
32.17	40.43	1305	0.009634
33.92	42.63	1257	0.010
60.91	76.54	855	0.01506
82.36	103.5	651	0.01931
105.2	132.2	525	0.02392

Columns I. and III. are taken from the record. M represents $\frac{H}{1.2567}$ and λ stands for 10 μ. Columns II. and IV. are obtained from these. The plot of the values of H and ρ so obtained are shown in curve I.

Cast Nickel at 15°C.

Observations published by Professor Rowland, *Philosophical Magazine*, No. 48, 1874, page 327.

H	μ	ρ
1.284	52.6	19.0 x 10^{-3}
2.685	80.8	12.38
4.514	165.1	6.056
5.612	198.5	5.038
7.078	217.8	4.591
7.752	220.6	4.533
9.076	222.3	4.50
11.54	218.2	4.583
13.94	204.7	4.885
17.29	187.8	5.325
19.53	177.3	5.640
22.95	162.8	6.143
27.59	148.1	6.752
41.52	111.9	8.937
72.70	72.5	13.97
104.2	53.4	18.73

Columns I. and II. are taken from the record. The plot of columns III. and I. is shown in curve II.

Cast Nickel at 12°C.

Observations by Professor Rowland, published in the *Philosophical Magazine*, for 1874, No. 48, Table II, page 328.

H	μ	ρ
2.325	53.55	18.67×10^{-3}
4.769	163.8	6.124
5.778	198.3	5.043
7.343	218.5	4.577
8.823	224.3	4.458
10.73	219.2	4.562
15.38	196.1	5.10
20.63	174.0	5.747
29.64	139.4	7.174
42.18	110.3	9.061

Columns I. and II. are taken from the record. Column III. is calculated from II. The plot of columns I. and III. is shown in curve II. in black circles.

Cast Nickel at 220°C.

Observations by Professor Rowland, in continuation of above Table III, page 328.

H	μ	ρ
2.26	199.2	5.02×10^{-3}
4.506	310.8	3.218
5.296	314.6	3 178
6.742	301.1	3.321
8.069	279.3	3.580
10.64	248.3	4.027
15.08	203.8	4.907
19.10	175.6	5.695
29.48	129.9	7.698
55.36	77.0	12.99
78.98	55.6	17.92
1.30	118.2	8.46
2.237	190.5	5.25
2.515	212.2	4.713
3.319	285.8	3.50
4.328	313.6	3.19

Columns I. and II. from the record. III. calculated. The plot of I. and III. is shown in curve III.

Observations by Professor Ewing, as published in the *Electrician*, Lon., May 16th, 1890; vol. 25, page 31. Since these observations are shown in the form of curves without the original numerical data the following fifteen tables are based on measurements upon the published curves. B and μ are thus measured H. $B—H$ and ρ calculated.

ANNEALED NICKEL ROD. NORMAL.

	B	μ	H	B-H	ρ
Curve IV.	0	40			
	100	50	2.	98	20.4 x 10^{-3}
	420	100	4.2	416	10.1
	720	150	4.8	715	6.712
	1100	200	5.5	1095	5.028
	1500	225	6.667	1493	4.465
	2080	200	10.4	2070	5.025
	2500	150	16.66	2483	6.708
	2960	100	29.6	2930	10.1
	3500	70	50.	3450	14.5
	3920	50	78.4	3842	20.4
	4500	32	146.	4354	33.53
	5000	22	227.	4773	47.56

Same annealed rod under stress of 3.5 kgs. per sq. mm.

Curve V.	500	120	4.167	496	8.40
	700	150	4.61	695	6.634
	1000	195	5.128	995	5.153
	1450	250	5.80	1445	4.013
	2000	275	7.272	1993	3.648
	2500	255	9.80	2490	3.937
	2870	200	14.35	2856	5.025
	3000	180	16.67	2983	5.587
	3500	115	30.43	3470	8.770
	4000	72	55.56	3944	14.09
	4500	48	93.74	4406	21.28
	5000	27	185.2	4815	38.46

Same annealed rod under stress of 6.8 kg. per sq. mm.

Curve VI.	400	120	3.333	396.7	8.404
	500	140	3.57	496.4	7.192
	750	175	4.29	745.7	4.752
	1000	220	4.55	995.5	4.571
	1200	250	4.8	1195.2	4.016
	1500	292	5.14	1495.	3.438
	2000	333	6.0	1994.	3.008
	2300	347	6 67	2293.	2.908
	2500	340	7.35	2493.	2 948
	2750	335	8 21	2742.	2.994
	3000	310	9.68	2990.	3.238
	3500	240	14.58	3486.	4.182
	4000	140	28.6	3970.	7.2
	4250	100	42.5	4207.	10.1
	4500	77	58.4	4442.	13.15
	5000	42	119.1	4881.	24.40

Rod of Unannealed Nickel Normal.

Electrician, London, Oct. 31st, 1890, vol. 25, p. 732.

	H.	I	$4\pi I$	ρ
Curve VII.	16.7	67	842	19.84 x 10^{-3}
	20	96	1206	16.58
	27.5	150	1885	14.59
	30	165	2073	14.47
	36	200	2513	14.33
	40	215	2702	14.81
	50	250	3142	15.92
	60	270	3393	17.68
	70	292	3670	19.08
	74	300	3770	19.63
	80	310	3896	20.54
	90	323	4059	22.18
	100	340	4273	23.40
	110	354	4448	24.73
	120	365	4587	26 16
	130	376	4725	27.51
	140	388	4876	28.71
	150	400	5026	29.85

Same rod under stress of 1.9 kgms. per sq. mm.

Curve VIII.

13.2	50	628.4	21.0
16.7	100	1257	13.28
22.2	150	1885	11.78
29.6	200	2513	11.78
40	250	3142	12.73
58	300	3770	15.39
70	322	4047	17.30
80	338	4248	18.83
90	351	4411	20.41
100	362	4549	21.98
110	372	4675	23.53
120	381	4787	25.07
130	390	4901	26.52
140	400	5026	27.85

Same rod under stress of 3.5 kg. per sq. mm.

Curve IX.

H	I	$4\pi I$	μ
10	28	3518	28.43 x 10^{-3}
11.8	50	628.4	18.77
14.5	100	1257	11.52
18.2	150	1885	9.655
20	165	2073	9.644
24	200	2513	9.55
30	245	3079	9.742
40	290	3644	10.98
50	317	3984	12 55
60	338	4248	14.12
70	352	4423	15.83
80	361	4537	17.63
90	371	4663	19.31
100	380	4775	20.94
110	390	4001	22.44
120	400	5026	23.88
130	407	5114	25.42
140	411	5165	27.10
160	416	5228	28.69

Same rod under stress of 6.8 kg. per sq. mm.

Curve X.

10	55	691.2	14.47
12.5	100	1257	9.944
14.4	150	1885	7.638
16.6	200	2513	6.606
20	240	3016	6.631
27	300	3770	7.160
39	350	4398	8.866
50	372	4675	10.7
60	388	4876	12.31
70	398	5002	14.0
80	402	5052	15.84
90	406	5102	17.64
100	410	5152	19.41
110	412	5178	21.23
120	419	5266	22.79
130	422	5303	24.51
140	426	5354	26.14
150	430	5404	27.75
160	432	5429	29.47

Same rod under stress of 10 kg. per sq. mm.

Curve XI.

H.	I	$4\pi I$	ρ
8	50	628.4	12.73 x 10^{-3}
10	100	1257	7.956
12	150	1885	6.366
13.6	200	2513	5.412
15.2	250	3142	4.838
18	300	3770	4.775
20	316	3972	5.035
25	350	4398	5.685
30	370	4650	6.452
40	392	4926	8.122
45	400	5026	8.952
50	404	5077	9.848
60	410	5152	11.65
70	412	5178	13.52
80	418	5253	15.23
90	420	5278	17.06
100	422	5303	18.86
110	425	5341	20.59
120	430	5404	22.21
130	434	5454	23.83
140	438	5804	25.43
150	440	5530	27.13
160	442	5555	28.8

Same rod under stress of 13.3 kg. per sq. mm.

Curve XII.	9	118	1488	6.07
	10	147	1847	5.414
	11.2	200	2513	4.456
	12.8	250	3142	4.073
	14	300	3770	3.718
	16.5	350	4398	3.752
	20	373	4687	4.266
	25	400	5026	4.974
	30	410	5152	5.822
	40	420	5278	7.58
	50	422	5303	9.426
	60	427	5366	11.18
	70	430	5404	12.96
	80	432	5429	14.74
	90	435	5466	16.47
	100	440	5530	18.08
	110	441	5542	19.84
	120	443	5567	21.55
	130	446	5605	23.19
	140	448	5630	24.87
	150	450	5655	26.53

Same rod under stress of 19.8 kg. per sq. mm.

	H	I	$4\pi I$	ρ
Curve XIII.	6.2	50	628	9.871×10^{-3}
	7.0	100	1257	5.568
	8.0	150	1885	4.244
	8.6	200	2514	3.421
	9.6	250	3142	3.055
	10	275	3456	2.894
	10.4	300	3770	2.758
	11.6	350	4398	2.64
	13.5	400	5027	2.685
	20	440	5530	3.616
	30	447	6618	5.340
	40	449	5642	7.088
	50	450	5655	8.840

For all further values of H, I constant at 450

	100	450	5655	17.69

COBALT.

Observations by Professor Rowland on ring of Cast Cobalt as published in the *Philosophical Magazine* for 1874, No. 48, Table IV, page 332.

	H	μ	ρ
Curve I.	4.933	87.24	11.46×10^{-3}
	5.883	95.34	10.49
	7.647	109.95	9.095
	9.315	124.8	8.013
	11.3	132.7	7.536
	12.93	134.9	7.413
	15.94	140	7.143
	18.90	141.6	7.062
	21.96	139.3	7.179
	26.47	134.2	7.452
	35 11	123.7	8.084
	40.0	116.6	8.576
	55.21	100.4	9.96
	73.21	86.6	11 55
	99.98	71.8	13 93
	147.1	54.9	18.21

Cast Cobalt Ring at 230 C, Table VI, page 333.

	H	μ	ρ
Curve II.	1.334	101.8	9.82×10^{-3}
	2.567	113.6	8.803
	3.855	128.2	7.8
	5.556	169.1	5.914
	7.516	210.2	4.757
	10.14	235.9	4.239
	13.27	235.5	4.246
	17.29	220.2	4.541
	28.18	186.4	5.365
	39.36	161.2	6.203
	70.29	117.0	8.547
	98.93	96.63	10.35
	128.2	82.87	12.07

Observations of Professor Ewing on a rod of Cast Cobalt, published in the *Electrician*, London, May 16th, 1890, vol. 25, page 31. The results appear in a curve from which the following measurements have been made.

	B	μ	H	$B-H$	ρ
Curve III.	750	115	6.52	74.3	8.776×10^{-3}
	1500	150	10.	1490	6.71
	2500	180	13.89	2486	5.586
	3000	190	15.79	2984	5.292
	4000	195	20.51	3980	5.152
	5000	192	26.04	4974	5.235
	6000	187	32.09	5968	5.375
	7000	161	43.48	6956	6.251
	7500	151	49.67	7450	6.668
	8000	140	57.15	7943	7.194
	9000	115	78.26	8922	8.772
	10000	92	108.7	9891	10.99
	10600	80	132.5	10467	12.66

Same rod under stress of 16.2 kg. per sq. mm.

Curve IV.	750	155	4.84	745	6.495
	1500	200	7.50	1492	5.03
	2500	240	10.42	2490	4.19
	2800	250	11.2	2789	4.02
	3700	260	14.23	3686	3.86
	4800	250	19.2	4780	4.02
	5000	242	20.66	4979	4.15
	6350	200	31.75	6318	5.02
	7500	160	46.88	7453	6.29
	9250	100	92.5	9157	10.1
	10000	77	129.9	9870	13.16
	10750	60	179.16	10570	16.95
	10450	65	160.7	10290	15.6

Iron in Powerful Magnetic Fields.

Observations by Ewing & Low, published in the London *Electrician*, July 25th and Aug. 1890.

Swedish Wrought Iron.

H	I	μ	B	$B-H$	ρ (metallic)
6,690	1700	4.18	27,960	21,270	0.314
8,900	1660	3.34	29,730	20,830	0.427
9,510	1700	3.24	30,820	21,310	0.446
10,000	1690	3.12	31,210	21,210	0.472
10,360	1700	3.05	31,630	21,270	0.488
10,810	1670	2.94	31,720	20,910	0.515
10,880	1690	2.95	31,060	21,180	0.513
11,200	1690	2.90	32,360	21,160	0.526

Lowmoor Wrought Iron.

H	I	μ	B	$B-H$	ρ (metallic.)
3,630	1680	6.80	24,700	21,070	0.1724
6,680	1670	4.13	27,610	20,930	0.3195
7,800	1680	3.70	28,870	21,070	0.370
8,810	1630	3.33	29,350	20,540	0.429
9,500	1650	3.18	30,200	20,700	0.459
9,780	1660	3.14	30.680	20,900	0.467
10,360	1630	2.98	30,830	20,470	0.505
10,840	1630	2.89	31,370	20,530	0.529
11,180	1620	2.82	31,560	20,380	0.549

Cast Iron.

H	I	μ	B	$B-H$	ρ (metallic.)
3,900	1250	5.04	19,660	15,760	0.2475
6,400	1240	3.42	21,930	15,530	0.4132
7,710	1200	2.96	22,830	15,12	0.5102
8,080	1230	2.91	23,520	15,440	0.5236
9,210	1220	2.67	24,580	15,370	0.5988
9,700	1210	2.57	24,900	15,200	0.6369
10,610	1190	2.46	25,600	14,990	0.6849

LOWMOOR WROUGHT IRON.

H	I	μ	B	$B-H$	ρ (metallic.)
24,500	1660	1.85	45,350	20,850	1.176

If at any point H = the magnetic force,
B = the flux.
I = the intensity of magnetization.
k = the susceptibility.
μ = the apparent permeability.
m = the metallic "
r = the apparent reluctivity.
ρ = the metallic.

$$\text{Then } B = \frac{H}{r} = \mu H$$

$$\text{And } r = \frac{H}{B} = \frac{1}{\mu}$$

$$\text{Also } B = H + 4\pi I = H(1 + 4\pi k)$$

$$\therefore \mu = 1 + 4\pi k$$

$$1 + m = 1 + 4\pi k$$

$$\frac{1}{\mu} = \frac{1}{1 + 4\pi k} = r$$

$$\frac{1}{m} = \frac{1}{4\pi k} = \rho$$

$$r = \frac{\rho}{1 + \rho}$$

$$\rho = \frac{r}{1 - r}$$

$$\frac{\rho}{r} = \frac{B}{B - H}$$

DISCUSSION.[1]

THE CHAIRMAN:—Gentlemen, you have all heard and most of us have also read this very interesting paper. The valuable character of the paper has, I am sure, been enhanced in the minds of most of us by the fact that the author was able to be present and himself read it to us.

Before calling upon the Institute for discussion, I would like to ask Mr. Kennelly if it is not true that upon page 490 the word "excepting," in the second paragraph, should have been "accepting?"

MR. KENNELLY:—I think, sir, the intent of that paragraph is to show that only the formulæ of Frölich and Lamont have been regarded as at all satisfactory.

THE CHAIRMAN:—Gentlemen, the paper is before you for discussion.

MR. CARL HERING:—The paper is exceedingly interesting and leaves little to be discussed, but there are one or two points I would like to speak about. It is a great satisfaction to see from this interesting paper that there is after all a straight line law in magnetic phenomena and that it is so for just that range of values which is used most in practice.

I think it would have been clearer to many readers if Mr. Kennelly had added that the reluctivity, ρ, which he uses, is the reciprocal of μ. This is inferred in the paper, but as we are usually more familiar with μ than with ρ, I think it would have been clearer, to some of us at least, if that had been stated.

In referring to his curve, Fig. 4, he says, "The reluctivity of iron appears from these curves to commence at a certain *definit*- and moderately large value." It seems to me it is a very indefinite value because according to the other curves it is the reciprocal of zero. That is to say, theoretically, this curve would start at infinity, and therefore it follows that the practical initial value would be a rather indefinite value. I am not sure whether I am right in that, if I am wrong, I hope Mr. Kennelly will correct me. It certainly does not seem to be a very *definite* value, because curve (Fig. 4) is the reciprocal of the curve shown in Fig. 2, which appears to start from or near zero. If this initial value is a definite one why is it not given in any of the Figs. 5, 6, 7 and 8?

Fig. 3 is not quite correct at the end of the curve, and is a little misleading on that account. The curve comes down as if it was going to cut the x line just beyond the figure; but that is not the case as there is a point of inflection there, shown by the two last plotted values. If it were to cut that line it would have values here, of a negative conductivity or a source of magnetism, which, of course, is absurd. If the values had been carried out farther, the line would tend to become tangent, I think, to a hori-

1. By Messrs. Hering, Brown, Bell, Steinmetz, Pupin, Lockwood and Emery.

zontal line a little above the x line. It is a little misleading and puzzling to any one to whom a curve expresses a law, to see this line start so definitely toward the x line as if it were going to get there.

Referring to Fig. 10, which gives Mr. Kennelly's interesting straight line, if I understand that the values of y are given in such units that the unit 1 near the top, represents the reluctivity of air, which I suppose is what was meant, it seems to me this line cannot continue to be straight. It would seem that it must tend to become tangent to the unity line; that it must curve off and never get above that line, for if it got above that line we would have a peculiar condition here, because, for such values of H it would follow that if you took the iron out of the field, the remaining field in the air would be stronger than when the iron was there. In other words, if you generate a field of that strength with some iron in it, and then take the iron away, the field will be stronger than it was before. I did not have time to study the paper, I merely read it over very hurriedly, and if I am wrong in this I hope Mr. Kennelly will correct me.

I think it would have added to the interest a little to have marked on these curves the values at which iron was usually used in practice, so as to give us an idea of what parts of the curves are most important. In Figs. 1 and 3 it corresponds to values in the neighborhood of 15,000; in Figs. 2 and 4 to values of $H = 10$. In Fig. 2 it is about *there*, [indicating], and it is a somewhat curious condition shown here that we use iron where it has an apparent conductivity much lower than its maximum—about $\frac{1}{8}$ of its maximum apparent conductivity.

It might also have been interesting to have added to these curves the corresponding curve for air, in order to show the relation between them. In Fig. 1 the curve for air would be a straight line very close to the x line, making a very small angle with it. In Fig. 2 the line for air is a horizontal line parallel to the x line, and I think it is the line which this curve approaches as an asymptote. In Fig. 3 the line is also a horizontal line very near the x line. In Fig. 4, it is also a horizontal line, but its location, according to the scales used, would be very far above this diagram.

Near the end of his paper, Mr. Kennelly refers to an analogy with an electric circuit containing an electrolyte. That analogy appears to me to be a very interesting one, and I think deserves a little more attention. It may perhaps assist in understanding some of the phenomena. If, for instance, we take two platinum plates in dilute sulphuric acid and subject them to a difference of potential, beginning at zero and gradually increasing, plotting the value of the E. M. F. as X, and those of the current as Y, we will get a curve very similar to Fig. 1. It will differ slightly at the origin; it will be a little steeper in the ascent, and a little sharper at the bend. In this analogy the E. M. F. corresponds to

H; the current corresponds to B; the apparent resistance corresponds to ρ, and the apparent conductivity to μ. Carrying out this analogy here, we will find that the liquid will give curves almost exactly like those in Figs. 1, 2, 3 and 4. The curve approaches a horizontal line as an asymptote just as the curve in Fig. 2 does. It starts like that in Fig. 2, has a maximum near the Y line, and comes down in the form of a hyperbola. If this curve is worked out you will find it to be almost exactly like those shown in Figs. 1, 2 and 3. It will also be found to be very similar to that in Fig. 4, only that the line will not be straight. I did not have any definite values from which to work out the curves of this analogy, but assuming the first curve to be something like the one shown in Fig. 1, which will undoubtedly be the case, I worked out all the others, and found they agreed very nearly with the magnetic curves. [Applause.]

MR J. STANFORD BROWN:—Mr. Kennelly has spoken of the term "magnetic resistance" on page 489, and of certain objections which have been made to that term. Authorities differ somewhat on the definition of reluctance and it might be well to call attention to it. In other words, according to some, it is the ratio of magneto-motive force to the total magnetic flux. According to others it is the ratio of magneto-motive force to the flux density. I believe the first definition is due to Hospitalier in the beginning, and is followed by Prof. Fleming in his "Alternate Current Transformer;" while the latter has been followed by Bosanquet and others. If such is the case, it might be well for us to decide here, which is the best, and have the Institute adopt one or the other as the authoritative definition. It might give us a practical idea of it in another form to remember that in making use of this relationship in motor work, or dynamo work, we are dealing with ampere turns and the force lines *per unit area.*

Another point of interest is on page 486 or 487, where the mathematical quantity 4π is introduced. Mr. Kennelly draws attention to the fact that it comes in where we are dealing with a surface containing a single unit value, whether that surface is a sphere or any enveloping surface. In one of the recent papers by Mr. Heaviside that factor 4π is spoken of, and he shows there that it does not of necessity enter as a physical quantity into the laws of the magnetic circuit. It has no physical significance, only a mathematical one, and that fact has always been a stumbling block to students. I do not know that I make myself clear about it, but probably others will bring the fact out more fully.

DR. LOUIS BELL:—With reference to the curves Figs. 2 and 3 in the paper just read, I may be able to give a word of explanation.

I remember seeing Prof. Rowland use the curve, Fig. 3, as a horrible example of the danger of extrapolation beyond the

experiments. He supposed as others supposed, at the time his work was done that the permeability fell down to zero, that there was an end of the power of added force to produce effect; that permeability rose to a certain point and then dropping and stopped there; and it was something of a revelation to everybody when the investigations of Ewing showed that the curve became asymptotic. I had the pleasure of hearing Prof. Ewing read one of the classical papers on the subject at Manchester some three years ago, and it was generally remarked as surprising that the curve did become completely asymptotic. The interpretation to be put then on that particular diagram is that a man has no business to continue the deduced curve beyond the point where his experiments stop, unless he knows what is going to happen, which he seldom or never does.

Mr. Chas. P. Steinmetz:—I cannot agree with the opinion expressed here, that the curve of magnetic reluctance does not start from a definite value, but begins at an infinite value. On the contrary, I entirely agree with Mr. Kennelly that the ini al point of the curve of magnetic reluctance always must have a finite value.

For, if you consider the curve of dependence of the magnetism upon the magneto-motive force, the curve given in Mr. Kennelly's paper as Fig. 1., you see that this curve starts in the zero point with a finite angle, different from zero, and different from 90°.

Now the magnetic conductivity is the tangens-function, the magnetic reluctivity the cotangens function of this angle, and the angle being different from zero and different from 90°, both functions must have a finite value. That means: magnetic reluctivity as well as magnetic conductivity start from a *finite* initial value.

And, that the "magnetic characteristic" curve, Fig 1. of the paper intersects the axis of abscissæ under a finite angle different from zero and from 90°, is evident, and proved by the fact that, if you send an alternating current around this magnetic circuit, no matter how much you decrease this current, it will always encounter a *finite* amount of self-induction. Now self-induction of the alternating electric circuit measures the conductivity of its magnetic circuit, and therefrom you prove that this conductivity in the zero point, and thereby also its inverse value, the reluctivity is *finite*.

I am very much pleased to see that Mr. Kennelly prefers the term "magnetic reluctance" to the term "magnetic resistance," which already begins to be used rather confusedly. For electric resistance is one thing, and magnetic reluctance is another thing, and both things have got definite, but different dimensions, and so we have no right whatever to call them both by the same name, if we do not want to cause confusion. It would even be better not to have the term electrical resistance at all. Even

that is very misleading. It was introduced at a time when electrical science was still in a very crude state, and we knew nothing about the dimensions of these quantities, and so it came that the term "electric resistance" was introduced for that quantity, which determined the loss of energy caused by an electric current in the same way as in mechanical engineering the term "mechanical resistance" was already applied to that force, which consumes in every mechanical motion a part of the energy, by converting it into heat. But now we have the *mechanical resistance*, which is a true force, of the dimensions of *force*, while *electric resistance* is of the dimension of *velocity*, hence quite a different thing; and if we introduce *magnetic resistance* we have another resistance again, of still another dimension, a reciprocal length, and so we have the same name for three different quantities, of different dimensions, which is anything but commendable. There is besides an essential difference between this term "magnetic reluctance" and the two other resistances, because the two other resistances are quantities which really determine the consumption of energy or rather the conversion of energy into heat. But the magnetic reluctance does not convert potential energy into caloric energy. Hence if we want to use the term magnetic resistance at all we would more appropriately call another quantity with this name. In the magnetic circuit a certain consumption of energy takes place too, if the magnetism reverses. Hysteresis consumes energy. I think I shall be able at one of the future meetings to communicate to the Institute a number of experimental tests carried out in Mr. Eickemeyer's factory in Yonkers, on determinations of loss of energy by hysteresis, and its dependence upon the intensity of magnetization. These results seem to point to the fact that this consumption of energy by hysteresis can be expressed analytically by one coefficient, and this coefficient would, I think more properly be called magnetic resistance than any other quantity, because it is really that coefficient which determines the consumption of energy, that is the conversion of magnetic potential energy into heat; but it is better to avoid the name "magnetic resistance" altogether.

Another point I wish to mention. As known, elasticity is that physical force which is most analagous to magnetism, so much, that almost every phenomenon of elastic forces finds its analogy in the magnetic circuit. This analogy goes so far that, if Mr. Kennelly mentions that magnetism is able to store up in a given volume more energy than any other force, elasticity even beats magnetism: A twisted steel spring contains a greater amount of stored up energy than a highly magnetized piece of iron or a highly magnetized quantity of air. To mention only the analogon to hysteresis in the elasticity—when a steel spring is twisted it represents a certain amount of potential energy, like any magnetic circuit. Now, untwist this spring and it gives back energy, like a magnetic circuit, demagnetized, gives energy

back. But if you twist the spring and let it untwist itself, a certain amount of energy is not given back. Just so, if you magnetize a magnetic circuit by the expenditure of a certain amount of energy, which is converted into magnetic potential, and demagnetize it again, it does not give back all the energy, but a certain amount of energy is lost by molecular friction, by hysteresis.

Yes, this analogy goes still farther:

By twisting or untwisting alternately a steel spring, a certain amount of energy in each elastic cycle is converted into heat by external friction, and another amount by molecular friction.

The loss of energy by external friction per elastic cycle is proportional to the rapidity wherewith the cycle is completed, to the "frequency." The loss by molecular friction about constant, and independent of the rapidity of elastic changes.

Now in the magnetic circuit you have the same: the external friction of the elastic cycle—or the resistance caused by the work done by the spring—corresponds in the magnetic circuit to the induced currents, viz., eddy-currents set up by the alternating magnetism. Both are proportional to the frequency of the alternations, and depend upon outside conditions.

To the molecular friction of the elastic cycle corresponds in the magnetic cycle the molecular friction of hysteresis, both independent of outside conditions, and apparently upon the frequency. I need not to go farther into this comparison, for I shall have to dwell upon it more on another occasion.

Mr. Kennelly says that air has apparently no hysteresis, and this is the general assumption, too. But nevertheless, in the light of modern science we must say that even air has a certain hysteresis, a time-hysteresis. For we know now, that the magnetic stress in air does not appear instantaneously with its source; but we know that magnetic disturbances are propagated through air with a finite velocity, the velocity of light. Now, if you examine the phenomenon more particularly, you will see, that then, and only then, no energy would be dissipated in space, if the magnetic disturbance set up at any place, were propagated through the whole space instantaneously. But as soon as the propagation of energy through space consumes a finite time, no matter how small this time be, a certain loss of energy must necessarily be connected therewith, and, calling the retardation of the magnetic disturbance behind the magneto-motive force, hysteresis, we must say: even air has hysteresis. [Applause].

Mr. Chas. E. Emery:—I wish to thank the author for the very valuable information and suggestions contained in the paper, and to express my pleasure with the very scholarly way in which the subject has been presented. I will add a word in relation to the expression 4π, which has been discussed by one of the speakers. In investigating the formulæ for the construction of motors and dynamos it becomes quite evident that 4π is nothing

more or less than a simple arithmetical coefficient which, in this connection, has nothing to do with its customary significance as expressing the surface of a sphere of unit radius. It is thought that this should be emphasized by writing the figures instead of the symbol or, for convenience of calculation, substituting a single special character, for instance a, for it, and stating its numerical value. The artificial conception that a line of force radiates from a unit of surface of a sphere of unit radius, may be comprehended so far as the unit is concerned, but has not the slightest applicability in practical work. The conception causes great difficulty in the minds of students, and to it may be very well applied the language used on the second page of Mr. Kennelly's paper, where he refers to "the remarkable construction of purely artificial frame-works of thought void of all attempt at reality, yet capable of affording useful applications and exact quantitative results," etc. This particular language refers to theories in relation to magnetism, but the books contain very similar expressions in relation to 4π. It should be borne in mind that the criticism is not intended to apply to the electrostatic distribution of potential on the surface of a sphere or other solid, for in such case the surface of the sphere is one of the elements considered, whereas "magneto-motive" force, as it is called by the Committee on Units, has no necessary relation to a sphere or to a system of lines radiating in all directions from a common centre. The coefficient used in connection therewith, which has been called 4π, arises from the same causes that similar coefficients occur in almost all other formulæ, either rational or empirical. For instance, in the rational formula for the square of the velocity, we have in English units the coefficient $2g$. In the practical application of this formula certain resistances are frequently considered by substituting for $2g$ an empirical coefficient of less value. Some such coefficient is necessary in nearly all formulæ. In the absolute system of electrical units it is made unity and therefore disappears, but in many of the practical applications of such units a coefficient of different value than unity is a necessity. For instance, the value of g in absolute units must be used as a multiplier when the absolute units of force and mass are reduced to weight. The multiplier called 4π in the formula we are considering is required for a similar reason.

It is believed that the coefficient a can be properly called the mechanical equivalent of electromagnetic force and thereby correspond with the mechanical equivalent of heat. The value of the volt-ampere or watt is necessarily known in terms of mechanical work by definition, and is a multiple of the product of the absolute units of electromotive force and current. When, however, it is desired to substitute for the absolute unit of electromotive force in such product, the unit velocity of a conductor across unit field in a unit of time, a coefficient is necessary to reduce this modified unit of work to the customary unit of

work. This arithmetical coefficient, we call a, and its value has been fixed at about 12.6 or 4π, but from the fact that a certain multiple of π must be combined with 981, the acceleratrix of gravity in metric measure, it seems impossible that any exact multiple of π should be the result, and it is believed that on further investigation, the value will be modified and when this is done of course 4π must disappear, but the proposed constant, a, would be retained for the value finally determined upon.

It may be asked: how is it that a coefficient merely expressing an arithmetical relation between certain quantities has been referred to the surface of a sphere? It will be seen that is in some sense an imitation of the remarkable work of Weber and Gauss in referring the intangible phenomena relative to electricity to tangible measures of length, mass and time. The apparent object was to prevent the appearance of an arithmetical coefficient by clothing it in the garb of a direct measurement relating to a physical object. Such a mathematical parable could have been obtained by representing one line of force by one of the units of any length or any surface which contained or could be made to contain the total number of units desired; for instance very readily the tangent of an angle, when, if the angle corresponding to such tangent coincided with that between any notable lines of reference about the earth or in the heavens, for example, the dip of the magnetic needle or the inclination of the earth's axis or equator to the ecliptic, the physical relation would have been established. In this particular case, however, the number of units which it was desired to represent by a tangible well known object was equal to or could be called the number of units on the surface of a sphere of unit radius, so it was possible to follow the reasoning in relation to the distribution of potential and represent a line of force by the solid angle included by unit area on the surface of a sphere of unit radius, and as there were 4π or 12.6 units of area on the surface of such a sphere, the value of the coefficient was thus artificially fixed at 4π.

The objection to an artificial representation of this kind is evident. The mind is filled with conceptions which incompletely represent the phenomena and consequently proper courses of action under the particular circumstances do not suggest themselves as readily as if more natural and customary methods were employed. It is not so much the use of π as it is the artificial atmosphere which 4π brings. The π by itself has a definite arithmetical value, but 4π implies a conception which carries the mind into the clouds. The attempt to use any unusual units in practical work has also much the same effect. The mind involuntarily grasps and suggests relations between the units of length and capacity which are customarily employed in the country of the investigator. It unnecessarily cripples his capacity and occupies his time in a cataclysm of arithmetical work to attempt to regularly use one set of units when accustomed to another. For in-

stance, without deciding that metric are better or worse than English units, it is safe to say that a person should use those to which he is accustomed, for ordinary lengths and weights even in electrical work, but that all should use the same units for strictly electrical quantities. This may appear contradictory, but the result is easily accomplished by using formulæ in the general form employed in all other branches of engineering, or of the kind in which an arithmetical co-efficient precedes the characters which represent the relations to be considered. Every formula, both rational and empirical, takes the general form: The quantity desired equals A (an arithmetical co-efficient) multiplied by a function of a, multiplied by a function of b, multiplied by a function of c, etc. In electrical formulæ the functions are very simple, many of them being represented by the characters themselves, but even in such formulæ the initial co-efficient is of the greatest value. It may in some cases be equal to unity, but the character should be retained for reasons, yet to be stated It may combine in itself all the arithmetical relations of the several quantities, and, more valuable than all for electrical work, it may show the arithmetical relations of the quantities when expressed in different units; for instance, have one value (A) when all the quantities are in absolute measure, another (A_1) when all are in English measure, but better it can have still another value (A_2) when part of the quantities are expressed in ordinary electrical terms referring to metrical measure and the remainder in English measure. For instance, the magnetic field may be stated in c. g. s. lines of force in the same formulæ where other quantities refer to English feet, inches or pounds and yet the torque be brought out in pounds acting at the distance of a foot just as well as if the result were first obtained in dynes at the distance of a centimetre and then a process of arithmetic gone through to secure the final result. On the system proposed, the arithmetical work is all done once in giving the constants the proper value, after which the application of the formulæ is so simple that it may be trusted to an intelligent office boy.

The foundations laid by the distinguished men referred to, when establishing the absolute units, is admirable, but the artificial framework accompanying the same is too apt to delude men into believing that such a system secures absolute accuracy—in other words, is absolute in the complete sense of the word. The fact is, there is nothing absolute. Nature did not lay out her metes and bounds so that they would be exact aliquot parts or multiples of the units of men. The metre was supposed by some to be an absolute decimal division of the earth's quadrant. Now, it is well known that it is not, but that the earth's quadrant, referred to in metric measure, is an artificial one made to fit the metre. The metre is nothing more or less than the distance between two marks on gold plugs in a platinum bar, etc., and might just as well have been arranged at the same distance as the two

marks on the English bronze bar which fixes the length of the standard yard. Prof. J. Clerk Maxwell, in discussing the subject of the metre, says: "The authorized standard is therefore not the terrestrial globe, but Borda's platinum rod which is much more likely to be accurately measured." It must not be thought that the use of absolute measure in electrical formulæ for proportioning dynamos and motors secure absolute accuracy. There is apparently no numerical coefficient at the beginning of such formulæ which can be modified to suit the conditions, but corresponding allowances are necessarily made at another stage of the calculation, for the reason that the number of magnetic lines of exciting force must be from 20 to 40 per cent. in excess of the number that are utilized across the armature, so although the formulæ are absolute in form, the results must be modified as in other branches of engineering by a factor which Towle, the engineer who saved the "Great Eastern," said should be embodied in every formula. He called it K, and after giving many examples of its value and general applicability, used to explain that K meant common sense. [Laughter.]

These criticisms are only directed to the artificial and unreal features of electrical engineering, and it is believed that if the same were eliminated, the mind would be left in a better condition to grasp the important problems still left for the solution of the electrical engineer. [Applause.]

Dr. M. I. Pupin:—I once heard a lecture by a very famous specialist, (Prof. Winkler of the Berlin Polytechnic school), in bridge building and railroad building in general; it was a lecture on designing bridges. He said, if you want to be sure that your bridge will not break down under the load for which it is designed make it three times as strong as your calculations require. Well, as a rule that will never apply to electrical engineering. It would be very foolish to say to a man, If you want a one-horse power dynamo make it big enough for a three horse-power load. In the very nature of things this difference must exist. In mechanical engineering they do their testing in a way entirely different from that in which the coefficients and units are determined in electrical engineering. Electricity is nothing if not an exact science. Mechanical engineering is not yet a perfectly exact science. Our methods of testing are exceedingly exact and electrical units are derived by very exact and comparatively speaking very easy means. We cannot change them. Because a certain quantity has the numerical coefficient 4π we cannot complain of that. God has made the world in such a way that 4π multiplied by the square of the radius equals the surface of the sphere, and that is the reason that 4π appears in our formulæ. That is to say, 4π reminds us of a peculiar property of space.

It is an incommensurable quantity, and that is another bad thing about π, and yet this 4π appears all the time in electro-

magnetism. It appears on account of our definition of the fundamental units. We define our amperes and volts and ohms, by defining first our units of length, of time, and of mass which leads then to our definition of the units of force and work; we apply then Ampere's fundamental law in electro-magnetism and Coulomb's fundamental law in magnetism, and Joule's well-known law to obtain our units of current, electromotive force, and resistance. Proceeding in this way we finally discover that, because God made space in such a way that 4π times the square of the radius is equal to the surface of a sphere, for that reason only and no other do we find that if you carry a unit pole once around a wire through which a current, C, circulates, that the work is $4\pi\ C$; whether you pass around in a circle or in any curve whatever, the work is equal to $4\pi\ C$. These few remarks perhaps may serve to point out why the 4π comes up. It is simply due to our units. But we cannot go back and change everything in order to get rid of this 4π, because this 4π will come in somewhere else anyhow.

A gentleman remarked with reference to hysteresis—this has nothing to do with Mr. Kennelly's paper, but still I think I will venture to comment upon it—a gentleman remarked that the magnetic disturbance needed a certain time to pass from one place to another. Hence there must be a magnetic hysteresis in air. I do not think this proof a good one at all. Of course, we know that light needs a certain time to pass from the sun to the earth, and yet the light energy is not converted into heat until it strikes a terrestrial body, which it would be if its finite velocity through ether implied something like a hysteresis. As long as the light energy is in pure ether it consists of what the Germans call "*geordnete Bewegung.*" I cannot give you this term in English. It means a motion in a finite portion of space, such that it varies from point to point, continuously, according to some definite analytic law. Now, the motion corresponding to heat does not follow any definite analytical law, as we pass from point to point in the body. As soon as a motion passes from the motion of the first kind to that of the second kind, it is dissipated into heat. Now, the light motion is an analytical motion, if I may be allowed to call it so, as long as it is in the ether. That is, there is not a particle of it transformed into heat, but as soon as it strikes a terrestrial body, then a part is turned into heat; but still even in the ether, light motion is propagated with finite velocity. It follows that although there may be no dissipation of magnetic energy into heat in the air, still magnetic disturbances would need a certain time to pass from one place to another. I believe, however, that some experiments have proved that there is some slight magnetic hysteresis in the air, but I am not sure on this point. Plucker (Wiedemann, Electr. und Magn., vol. iii., page 865) proved to a certain extent that there is a certain magnetic hysteresis in gases. I think that Prof. Trowbridge of Harvard

College has tried to prove or has proved that there is a magnetizing hysteresis in the air.

As far as Mr. Kennelly's paper is concerned. I want to call attention only to one point, and that is on page 498, where he says: "Following the vein theory, the polarization of the iron is then complete, and the intensity of magnetization or magnetic matter per unit cross-section of veins finds its maximum, so that while the total flux can go on increasing indefinitely, it can only do so by adding to the permeating field flux, the vein flux having reached its full limit." This is a sentence which is not perfectly clear to me in whatever of the various ways mentioned by Mr. Kennelly I look upon it, and I think that it needs an explanation and perhaps a fuller discussion. I am sure Mr. Kennelly will kindly oblige by telling us a little more concerning this one point.

Mr. Emery:—Mr. Chairman. I am very much obliged to the last speaker for reiterating his standard of faith in regard to the absolute derivation of 4π. It is true, as he says, that he cannot help the fact that the surface of a sphere is 4π, but I am sincerely sorry if he thinks that that fact has anything to do with magnetomotive force, I understood the speaker to start to say, that it was an accident that the coefficient equalled the surface of a sphere, and am sorry that he corrected himself. In this connection, we will call attention to the fact that it has been clearly pointed out that the magnetic result of the passage of an electrical current through a conductor, may be represented by what has been termed its magnetic shield, or the surface of a cylinder of unit radius and unit altitude. The area of such a surface, or the length of one turn of a conductor occupying it is 2π, and attention has been called to the fact that this must be multiplied by a coefficient to give the true magnetic attractive or repulsive effect in the direction of the conductor. This coefficient happens to be about 2, so we again get 4π, but it has nothing to do actually with the surface of a sphere. The conception of a cylinder increasing in length proportionally to the increase of length of the conductor is quite simple, but is not so with a sphere. In fact, the elongation of a sphere would only develop a cylindrical surface.

Mr. Steinmetz:—I want to say that in speaking of the time hysteresis of air, I only intended to draw the attention to the fact that even in a magnetic circuit entirely contained in air, a certain dissipation of energy takes place—that is, a certain amount of energy is not given back to the electric conductor. But what happens to this energy I did not mention at all. I only used the word "hysteresis" in its original meaning. Besides, even in the magnetic iron circuit, a part of the hysteretic loss of energy does not come out as heat, but as sound waves.

I can not at all concede that we always have to make allowances of twenty per cent., or anything else. I think when we have to make allowances, this only proves that we do not know

yet every constant as far as it is to be wished. Hence in the early times of electrical engineering, we had to do more than to make only an allowance of twenty per cent. Then it was that, for instance in dynamo building, we had to wind first a wire on the armature, run the dynamo and measure what it did, and after that lay out the right winding. I hope nobody does such a thing any more. But still you make a certain allowance in designing dynamos, though in some of the best types of iron-clad dynamos these allowances are so small that they can well be neglected, and we calculate the dynamo exactly; and where we know and understand the phenomena which we have to deal with quite fully we need to make no allowance at all, but are able to calculate the apparatus quite exactly. For instance, in transformer designing I am absolutely sure that at least in some types of transformers, if I have a sample of the iron given to me beforehand, that I can test its magnetic properties, I can lay out any transformer, and there will be found between the calculated and the actually observed values not a difference of one per cent. [Applause.]

THE CHAIRMAN:—If there are no others who wish to discuss this, we should be glad to hear anything that Mr. Kennelly may care to add.

MR. KENNELLY:—Mr. President and gentlemen, I think it would be better perhaps to clear up the questions which have been raised, with the aid of the blackboard, on these four figures. It was mentioned, I think, that the initial reluctivity could not commence at a definite value, because, if I mistake not, Fig. 2 showed a starting point from zero. Fig. 2, it must be confessed, is erroneous in that respect. The first observation is shown by a black mark, and the line should not be carried back to the zero point. As regards Fig. 3, that does not terminate at the base line. It ends with the two little dots that you see there, and any person is at liberty to continue that line (page 492) in any direction whatever. The exact direction, of course, will depend upon how you construe the word permeability; whether you mean metallic permeability, or permeability in the mass. There is a difference on the vein theory, because we have seen that the flux is supposed, on this theory, to be the magnetizing flux added to the flux in the metal itself. It dissociates what goes on in the metal from what goes on in the space occupied by the metal, while the ordinary term, permeability, includes both.

In regard to Fig. 4 and also Fig. 10 in the paper, it is quite true that in reluctance, as ordinarily defined, namely, as the reciprocal of permeability, $1 / \mu$, the line could not go beyond unity, and would stop before the second line from the top of the diagram, Fig. 10. But that is mentioned on page 497. It is also pointed out that if we borrow from the vein theory, the conception that the reluctance in the mass, is the joint reluctance of two things in multiple arc, namely, the metal and the space occupied by the metal (it is an artificial hypothesis—granted; but it

leads to a result which is easy to handle), one becomes an asymptote, the other, so far as we know, goes on forever; that is to say, it does not terminate at 24,500. Now, the reluctance of iron is shown at 24,500 to be a little over 1.1—call it 1.2. The reluctance of air is supposed to be constant, namely, unity. The joint reluctance of these two is

$$\frac{1.2 \times 1}{1.2 + 1} = \frac{1.2}{2.2} = 0.545.$$

Now, you see, .54 would be the apparent reluctance of the iron. It shows how very vital a change is introduced by that conception borrowed from the vein theory, for on the conception that there is a metallic reluctance, the line becomes straight. The whole object of the paper is to show that whereas the curves of flux in terms of magnetizing force, or of permeability in terms of magnetizing force, or of permeability in terms of flux density, are all complicated curves—they are curves which you would not like to define by any particular formula, the curve which follows from the conception of the inverse of permeability is, fortunately, a very simple one. It is one that although it may have no place in nature, happens to suit our purposes in a high degree. On Fig. 9, the dots there which show the observations, indicate that the reluctivity did commence at a definite value, namely, 0.75, in that particular example.

Now as regards the point that was mentioned of the vein flux having reached its full amount, that arises, does it not, from the fact that the total induction—the total number of lines of force in the metal per square centimetre of section can go on at least as far as 45,000. Whereas the intensity in the iron, that is to say, on the vein theory, the amount of magnetic material that you would discover by making a cut at right angles to the lines, the amount of magnetic fluid would reach a finite value, in other words the induction B increases indefinitely while I, the intensity of magnetization, reaches a maximum.

There is only one other point, I think, and that is about this unfortunate 4π. It is a matter sincerely to be regretted that we cannot in dealing with the simplest magnetic circuits determine the flux from the magneto-motive force, without the introduction of that very unfortunate co-efficient. As has been clearly pointed out by several speakers, it stands in the light of no more than a numerical factor, and is derived from the fundamental fact that the unit of quantity in magnetism is that quantity which would repel its equal with unit force at unit distance.

It would certainly be possible to suppress 4π from the practical side of the work. We could cause 4π to disappear from our equations of electromotive force and flux, but it would turn up in other places. If we force it out of the door it would come in at the window. The first effect would be that we would have to change all our ohms and volts. That is a task which is surely

enough to deter almost any advocate of such a change. [Applause.]

THE CHAIRMAN :—Gentlemen, we have heard a most interesting discussion upon this most interesting and valuable paper I think that we have to congratulate ourselves that we are so fortunate as to have papers like this presented and that discussion is taken up with as little reluctance as this has been. I have myself been very much interested in hearing this discussion respecting 4 π. It is not a new discussion by any means. There has always been a difference of opinion as to the value of π whether it had the prefix 4 or not. But I rather think after hearing both sides that the balance of opinion indicates that we shall have to continue the π diet for a little while longer.

I was particularly impressed with the remark dropped by Mr. Steinmetz in which he spoke of the impropriety which we have all committed ourselves to, following therein the custom of our predecessors for so many years, of speaking of "electrical resistance," which we had no business to do. For a long time it has been with me a pet hobby that it would have been much better if we had accustomed ourselves from the beginning to speak and think of electrical conductivity. I suppose this is the only case in the whole realm of science in which we speak of a quantity from the negative aspect, so to speak. None of us would ever think of measuring, say, the capacity of a tubular conductor by the resistance which water experienced in blowing through it, and if we had started in on conductivity instead of thinking of resistance, which as we all know is nothing but the ratio between the electro-motive force and the current, it would have been easier for us in many other ways.

It now becomes my pleasant duty, although it has not had so many precedents as it might have had, to propose a very hearty and cordial vote of thanks to Mr. Kennelly for his paper, and without waiting for a seconder I shall put that vote of thanks.

[The vote of thanks to Mr. Kennelly was adopted.]

THE CHAIRMAN :—Professor Ryan's paper, as many of you are aware, had a very slim discussion at our last meeting and the time has been so well advanced this evening that I do not know whether you will care to take it up now. I will, however, say that Prof. Ryan has forwarded certain corrections which will be read, and then I will ask the Institute to decide whether to continue the discussion at this time on Professor Ryan's paper.

[The corrections to Professor Ryan's paper were read.]

DR. HERING :—As Professor Ryan is not here himself, perhaps it might be better to postpone the discussion. It might be well, if we have time now to consider the report on magnetic units.

THE CHAIRMAN :—I think, perhaps, we have sufficient time to take some action on that. It is only a little after ten o'clock.

MR. HERING :—I will make a motion, if it is necessary, to postpone the discussion of Professor Ryan's paper.

[The motion was carried.]

THE CHAIRMAN:—Will some gentleman move now that we proceed to discuss the report of the Committee on Units and Standards.

MR. C. O. MAILLOUX:—I make that motion.

[The motion was carried.]

REPORT OF COMMITTEE ON UNITS AND STANDARDS.

ORANGE, N. J., June 20th, 1891.

TO THE PRESIDENT AND COUNCIL OF THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

GENTLEMEN—Your Committee on "Units and Standards" begs to submit to your attention the following report:

Your committee, considering that authorized and recognized names for four practical electro-magnetic units, at present unentitled, are needed by electrical engineers in this as well as in other countries, for dealing conveniently with magnetic circuits in analysis, discussion, and design, recommends to the Institute the four units as appended in detail, of magneto-motive force, reluctance, flux, and flux-density, in the hope that if favorably considered, the Institute may further the endeavors of the next International Electrical Congress towards securing for them universally recognized titles.

Yours very respectfully,

A. E. KENNELLY,

Chairman.

LIST OF NEW UNITS PRACTICALLY NEEDED FOR CONVENIENCE IN DEALING WITH MAGNETIC CIRCUITS.

1st. *Magneto-Motive Force;* or difference of magnetic potential.

Simple Definition.—The analogue in a magnetic circuit of voltage in an electric circuit.

Strict Definition.—The magneto-motive force in a magnetic circuit is 4π multiplied by the flow of current linked with that circuit.

The magneto-motive force between two points connected by a line, is the line integral of magnetic force along that line. Difference of magnetic potential constitutes magneto-motive force.

Electro magnetic dimensional formula, $L^{1/2} M^{1/2} T^{-1}$.

The absolute unit of M. M. F. is $\frac{1}{4\pi} \times$ unit current of one turn.

The practical unit is $\frac{1}{4\pi} \times$ ampere of one turn, or one-tenth of the absolute unit—*i. e.*, 0.0796 ampere-turn gives

the unit. The prefix kilo- would perhaps be occasionally used for practical applications.

2d. *Magnetic Flux.*

Simple Definition.—Total number of lines of force or total field.

Strict Definition.—The magnetic flux through a surface bounded by a closed curve, is the surface integral of magnetic induction taken over the bounded surface, and when produced by a current is also equal to the line integral of the vector potential of the current taken around the boundary.

The uniform and unit time rate of change in flux through a closed magnetic circuit, establishes unit electromotive force in the circuit.

Electro-magnetic dimensional formula, $L^{\frac{3}{2}} M^{\frac{1}{2}} T^{-1}$.

The absolute unit is one C. G. S. line of induction.

The practical unit is 10^8 C. G. S. lines.

Fluxes range in present practical work from 100 to 100,000,000 C. G. S. lines, and the working units would perhaps prefix milli- and micro-.

3d. *Magnetic Intensity*, or induction density.

Simple Definition.—Flux per sq. cm.

Strict Definition —The induction density at a point within an element of surface is the surface differential of the flux at that point.

Electro-magnetic dimensional formula, $L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1}$.

Absolute unit, one C. G. S. line per sq. centimetre.

Practical unit, 10^8 C. G. S. lines per sq cm.

In practice, excluding the earth's field, intensities range from 100 to 20,000 lines per sq. cm., and the working unit would perhaps have the prefix milli- or micro-.

4th. *Magnetic Reluctance.*

Definition.—Unit reluctance in a magnetic circuit permits unit magnetic flux to traverse it under the action of unit magnetomotive force.

Dimensional formula, $L^{-1} M^{0} T^{0}$.

The practical unit is 10^{-9} the absolute unit.

Reluctances vary in present practical work from 100,000 to 100,000,000 of these practical units, so that the working unit would perhaps employ the prefix mega-.

Discussion.

Mr. Hering:—Regarding this report I think the work is very creditably done and that we can be very glad that our committee is doing such good work. I think, however, that there might just as well have been added a fifth unit which expresses magnetic conductivity or permeability, in order to complete the series. It would be the reciprocal of the fourth. It has been left out entirely in the report. Perhaps there is some reason for having left it out.

In the beginning of the report, in the second paragraph it says: "The names for four practical *electro*-magnetic units, etc." I think it would have been a little clearer to have called them simply *magnetic* units, because the second, third and fourth units are entirely independent of any electric current. I understand that the term *electro magnetic* is probably used as a distinction from *electro-static units*, but I think it might be written in a little different way, so as not to produce the impression that they are in any sense electric units, because they are not; the majority of them are purely magnetic units.

Mr. C. O. Mailloux:—I endorse the remarks of Mr. Hering, as to the creditable manner in which this work has been done by the committee. I would take exception only with regard to some of the practical units, so-called, that is to say, the practical values. That is a question which interests greatly those who have to deal with the use of these practical values.

There is room for discussion as to what values are the most expedient to give to the practical units. The committee has seen fit to use as the unit of magneto-motive force, $\frac{1}{4\pi} \times$ ampere of one turn, which is, I think, a convenient practical value. Now, we can assign any values to the other two units if we assign them in the proper reciprocal relation, and still be able to use the formula analogous to Dr. Ohm's. In other words, we would be able to write: The flux equals the magneto-motive force divided by the reluctance, and we could have that relation maintained for almost any sets of values of the magnetic flux and reluctance. The committee has given the practical unit of flux the value of 10^8 c. g. s. lines, which involves, of course, giving to the reluctance the reciprocal value of 10^{-9}; and the magnetic intensity has been made 10^8, the same as the magnetic flux. Now, the practical unit of one hundred million c. g. s. lines per square centimetre appears to me rather high, inasmuch as the values we deal with practically are seldom over 10,000 or 15,000; and as it is admitted that 25,000 to 30,000 are satural values, I do not see the logic of using a value so entirely out of reach and calling it a practical unit. It is true that we could use the term "micro;" at the same time it seems to me we might just as well shorten the nomenclature by using a practical value more within industrial

bounds. The practical unit of flux, 10^8, would seem to be taken from its relation to the other units, such as the volt. I think we could sacrifice to some extent the convenience of a strictly uniform system in order to obtain convenience in other respects. I have to manipulate these several quantities a great deal in connection with the design of electrical machinery, and I have been in the habit for some two years past of using a unit of flux which is one million C. G. S. lines, or 10^6, which I find to be very convenient because it is a quantity which you meet with very constantly in practice and its use does not involve too many decimals or too large a figure. At the same time, as we want to have something which fits all the requirements and which applies as nearly as possible to all cases, I would be in favor of further reducing the values of the practical unit of flux to 10^4, or 10,000 C. G. S. lines, and of taking the corresponding value of 10^{-5} for magnetic reluctance which still preserves the mathematical relations of the formula analogous to Ohm's, while at the same time it gives us values within ordinary limits. Then in dealing with flux we could use the terms "deca" and "hecta," while the unit of magnetic intensity would be about right, 10,000 lines per square centimetre, a value constantly met in practice and which one could handle without needing any prefix, such as "milli" or "deci," or any other term. It seems to me this is a matter which wants to be very carefully considered. We are going to assign values to these units not for one day, but forever, so far as we know, and we want to be cautious and consider well that we do it in a manner that will be of the greatest convenience to the greatest number.

THE CHAIRMAN:—If the Institute will pardon a suggestion, perhaps at this conjuncture it would be well to take a vote on accepting the report, and then, if the Institute please, we may consider it in sections and dispose of it as may be thought fit.

MR. HERING:—Is not this a portion of a much longer report? I understand that the Committee on Units and Standards is considering other questions and that this is only a portion. Perhaps it might be well to postpone it.

THE CHAIRMAN:—It would seem that the report is practically complete as far as it goes. It is signed by the Chairman of the committee and we would like to hear from him as to what the intention of the committee was in submitting this.

MR. KENNELLY:—The report as submitted by the committee is complete in itself upon that head.

THE CHAIRMAN:—It would be very proper, it seems to me, to accept the report if any one would make that motion.

MR. HERING:—I make the motion.

[A member seconded.]

THE CHAIRMAN:—It is moved and seconded that the report be accepted with thanks.

[The motion was carried.]

THE CHAIRMAN:—Now, how will you take it up? Will you consider one of these units at a time, or will you act definitely upon it this evening at all?

MR. MAILLOUX:—It seems to me there is only one part upon which there can be any divergence of opinion. The rest is so well done that I think it is above criticism. I do think, however, that the practical values of the unit ought to be further considered and discussed, before we commit ourselves to them, for the reason that I have already given. Those who have to deal with these things in a practical way, are very much interested in having values that will obviate a cumbersome nomenclature—in having values which have, as much as possible, a physical significance. For instance I would be very much opposed to a unit of magnetic density which is physically inconceivable, like the farad for instance, unless taken with an inseparable prefix, micro, just because mathematical uniformity or symmetry is insisted upon. We may perhaps find, if we seek, something that is palpable and tangible, instead. Therefore I move that the report be adopted with the exception of the value of the practical unit.

PROF. FRANCIS B. CROCKER:—That point has already been discussed in this room very thoroughly in connection with the unit of inductance. If Mr. Mailloux's suggestion could be carried out we should all be very thankful, but it is probably impossible. We now have the farad as a unit, however awkward it may be, and we must use the microfarad for ordinary practical work. The farad must be related to the coulomb and the volt directly in the ratio of 1 to 1, for if it had a relation of a millionth to 1 it would introduce a still worse objection to the system of units. In that respect the units are perfect. Hence, when we come to the magnetic units we must simply follow this rule. The farad is more awkward than any of these new units and yet we have had it for a long time and it would be undesirable to change it. In fact, I think any one who carefully considers this matter will say it is out of the question to change the farad, and the same thing is true of these proposed magnetic units. The members of the committee are unanimous on that point. I have seen no one yet who has given this question careful attention, who has not become completely convinced on that score. I would call attention to the fact that the word "practical" as applied to these units, should be put in quotation marks. The farad is called the "practical" unit of capacity, because it is directly related to other units, the values of which were selected on account of their supposed practical convenience. We can select values for the first few units in a system, and the rest follow as a natural and inevitable consequence and any one who attempts to change them will get into still worse trouble. So that word "practical" should not be taken in any literal sense. It merely means a unit which is related to the volt or other fundamental units in the simplest ratio. We should have a third column in our table of

units, as I pointed out some time ago—*working units.* [1] Thus we have the absolute units, the so-called practical units and the real working units. The volt and the ohm were chosen to be both practical and working. But in the case of the farad this is not the case. In the case of the henry the "practical" value is very nearly a "working" value. In the case of the proposed magnetic units the two values are unfortunately quite different, but we cannot help ourselves.

MR. TOWNSEND WOLCOTT:—I agree with Professor Crocker in regard to this system business. It seems to me there are only two ways to make a practical system of units, either the way it has been done—to fix on the most important values and let the others come in as they will, or else go on the other tack and make all the practical units about the convenient size and let the discrepancy come in the equations. That is, introduce factors into all the equations to make them come right. It is always assumed that the factor has the power of 10. I think that most electricians would seriously object to the introduction of a constant into the equation $C = \frac{E}{R}$ because that is an equation which is used so much. We either have got to have the units of an inconvenient size or else introduce multipliers into some of the equations. I presume from what Prof. Crocker said that the units given by the committee here are all supposed to be the systematic practical units. But the unit of electrical density is 10^8 C. G. S. lines per square centimetre. The unit of length in the practical system is 10^9 centimetres, and the unit of area is consequently 10^{18} square centimetres, which makes the practical unit of density 10^{-10} C. G. S. units instead of 10^8. If you consider that this belongs to the practical system, you have got to consider that there are two units of length. We will call one the subjective unity of length or the imaginary one, and the other the objective unity of length by which we measure the magnetic circuits. Of course the centimeter is a very convenient length for measuring dynamo machines; it is just about right. But the square centimetre is not the systematic unit of area according to the practical system of units.

PROF. W. A. ANTHONY:—I just want to say one word in regard to this discussion that has been brought out on the value assigned to these practical units, and that is this: That in assigning the value of 1 to 1 we are following out the plan that has been adopted in assigning the values to other practical units that have already been adopted. If we do anything else than this we shall have a relation that we shall have to memorize. Now if we make our practical unit one which is a ratio of 1 to 1 and then apply a prefix that may be necessary in order to give a working unit, that prefix indicates the relation. We know that our prac-

1. Address before New York Electric Club, December, 1890.

tical unit has a ratio of 1 to 1. If we adopt 10^4 which gives the ratio 2 to 1 we have got to remember that it is 2 to 1, and by and by somebody will be making mistakes through not remembering whether it is 1 to 1 or 2 to 1 or 4 to 1. And then there is nothing in the name of the unit to indicate what its relation to the other unit is. Let us stick, I say, to the relation that has already been adopted, making the unit 1 to 1 and then adopting such a prefix as will indicate the relation of the working units to the practical units. I think there is another reason for this—that the electricians of the old world, I do not believe, will ever consent to any other arrangement, and this society of electrical engineers, although it is a society of a good deal of importance, cannot upset what has already been adopted by the electricians of the old world.

Mr. Hering:—I quite agree with Professor Crocker, and Professor Anthony and the committee. I think it would not do at all to break the continuity of this system, and I feel quite sure that if we take any different action the European electricians will not agree with us.

Mr. Brown:—With regard to the second paragraph—magnetic flux—the simple definition is given as "total number of lines of force," or "total field," and then further down it is referred to in absolute units as one c. g. s. line of induction. Of course you are all familiar with the distinction, as some desire to make it, between a line of force and a line of induction. I do not know whether it is best to consider that distinction here or not.

With regard to paragraph four, the heading is magnetic reluctance, and Mr. Kennelly's paper is headed the same way. I presume that in this report the word magnetic is introduced because magneto-motive force, and magnetic flux and magnetic intensity were used, but it would seem perhaps unnecessary. I believe that Mr. Heaviside proposed the single word "reluctance" to replace the term "magnetic resistance."

Mr. Hering:—Regarding the relation between the second and the third units, I think that this ought to be considered very carefully. The same question was brought up at the Congress this summer, when some of these units were discussed, and it was really the reason why they were not adopted at this Congress, because it brought about a doubt in the minds of some of the gentlemen as to what the true relation should be, whether it should be as 1 to 1 or whether the square of the linear dimension should be taken into consideration. At the Congress this question was brought up by Prof. Roiti, who is certainly very clear headed; much weight was put upon his remarks, and the whole discussion on magnetic units was dropped for want of time to consider this point. So I think we ought to be careful about what we decide regarding the relative value between the second and third units.

Mr. Wolcott:—The committee very carefully fixed the dimensions of these second and third units. If you figure it out on the

dimensions attached there, you will find that the third unit is not 10^8 but 10^{10}.

MR. BROWN:—There is one other point, Mr. President. In the third paragraph, magnetic intensity has the alternate meaning given it of "induction density," from which it might be inferred that one should use the term "induction" for the total number of lines of force or total field. It is quite customary to use the term induction, as "induction density" has been used in this report, and it has the authority among others of Prof. Fleming and Mr. Kapp. When it was proposed to use the term induction for the total number of lines of force, great objection was made to it in the British Institution of Electrical Engineers, by Prof. Silvanus P. Thompson. The usage was suggested at that time in a paper by Mr. Vignoles. Now that these points have come up here they had better be threshed out again, and the one or the other definition adopted by this Institute.

DR. PUPIN:—The first sentence in the definition of the electro-magnetic units reads magneto-motive force or difference of magnetic potential. I think there is a tendency now to distinguish between electromotive force and difference of electric potential. I am sure that it would be well to distinguish clearly between magneto-motive force and the difference of magnetic potential. The quantities magneto-motive force and electromotive force, refer always to the total circuit, whereas difference of potential refers to a part only of the circuit. I think it would bring in a desirable distinction between the two quantities which ought to be distinguished. I say a "desirable distinction" because I think there is a tendency now to distinguish them. I think that electrical engineers use the term "difference of magnetic potential" whenever they want to specify that, they do not refer to the total magnetic circuit—simply to a part of the circuit.

MR. STEINMETZ:—I think we ought to retain this name, "difference of magnetic potential." But we have to say:

1st. Magneto-motive force, *and* difference of magnetic potential. And in the definition:

The *difference of magnetic potential* between two points connected by a line is—

For, indeed, "magneto-motive force" and "difference of magnetic potential" are two distinctly different things. But both will be measured by the same new unit, just as "electromotive force" and "difference of electric potential" are both measured by volts.

Now, we want to introduce a new unit for these two magnetic quantities. But, then, we have to be complete and mention both in the definition, magneto-motive force and difference of magnetic potential.

THE CHAIRMAN:—From what has already been said I am quite clear that the suggestion I made was a good one—that we should consider these units one at a time. Then we should have made

some headway; but now I do not see that we are any further than when we started.

MR. KENNELLY:—If I may be permitted, sir, I would like to explain the action of the committee in reference to an important point that has been raised as to the relation between the second and third units. The third unit is the flux referred to the unit of cross-section. It is a question—shall it be flux density per square centimetre or flux density per square earth's quadrant? There is in the case of this unit the difference, that whereas all the other units are in the relation 1 to 1 and come into connection with each other continually, in formulæ and in practice, the magnetic intensity which is referred to the square section of circuit, is a subsidiary unit only. It is not a part of the main line. It is a side-track. We can afford to depart there, from the strict measure of the earth's quadrant without that danger of creating confusion which would certainly result if we were to adopt the centimetre in place of the earth's quadrant in the regular succession of "practical" units. One or the other has to be adopted. It is a matter for discussion. But the opinion of the committee was that it should be flux density per square centimetre. That would seem to be much more convenient. It seemed to give a unit which was not hopelessly too small, and although not according to the strict "practical" linear unit, the earth's quadrant, it was not a unit that would be likely to introduce confusion. It is true that the system of proposed units here exhibits a weak point. But it is also true that it is a moot-point and it probably can never be finally decided except by an International Congress. It may be that the Institute will prefer to attach to that particular clause a statement marking that a departure has here been made from the strict earth's quadrant unit of length, but it is a matter which is difficult to decide at present, and it fortunately appears in a unit of lesser importance.

MR. WOLCOTT:—I think that argument would be a great deal stronger if that departure from the system gave us a unit of a convenient size. The unit which it does give, is no more convenient than the other.

MR. KENNELLY:—Inasmuch as we use flux densities of 10.000 C. G. S. lines of force per square centimetre if we have 10^8 such lines as our "practical" unit we can easily come down, by the prefix milli-, but to build up 14 places would be a very serious consideration, if the "practical" density were 10^{-10} C. G. S. lines.

MR. STEINMETZ:—Perhaps it would be the best to drop this unit altogether, for it is not of so much importance as the other units, and has no analogue in the electric circuit anyway. There we have volts, amperes and ohms, but no unit for the current-density, but, when needed, we say "amperes per square millimetre," or "amperes per square inch," and so we might just as well in the magnetic circuit say "lines per square centimetre," as is done now.

MR. HERING:—I cannot agree with Mr. Steinmetz in this. I think that of the four units given here, the third one, namely: the one used to express the number of lines of force per square inch, or centimetre of field, is one of those which appear most frequently in practical electrical engineering. In discussing dynamo machines or transformers it is the intensity or density of magnetism which is among the most important data. In describing a dynamo we seldom have to mention the total number of lines in the field, but the density of the field is very often given.

DR. M. I. PUPIN:—But in the construction of a dynamo, we always express impressed electromotive force in terms of revolutions per second and the so-called effective field—that is the total maximum magnetic induction through one turn; so that if we construct the unit of induction in such a way as to be in harmony with our other practical units the rest would be all right. So far as the other unit is concerned—unit of magnetic intensity—we are never called upon to bring that into agreement with the other units, and therefore that unit can be brought in in any way to suit the convenience of electrical engineers. It would not spoil the harmony of the system if you make it 10,000 lines of force per square centimetre.[1]

PROF. ANTHONY:—It seems to me, Mr. Chairman, that we are figuring out the total lines of force by the number of lines of force to the square centimetre, and if we had some unit that was entirely at variance with the unit in which we express the total lines, we should have to use a multiplier, which we should probably forget.

MR. BROWN:—There is one point about this, if we begin with a small unit and then look at it as a whole, say in this case—the total area—that is really only looking at it from another point of view. I mention this because in a discussion which is being carried on in the electrical papers, with regard to a certain question of engineering, a misunderstanding has crept in on just this point. In that case the gentleman commenced with the unit of length—a foot—when regarding a certain circuit, and then he began to look at the circuit as a whole, and evolved certain deductions which were independent of the circuit length. The length of the circuit disappeared so far as the point in dispute was concerned. What I wanted to bring out is simply this: that it is really the same thing in the end, whether we look at the area as square centimetres or whether we look at the area as a whole. But it is always better to look at it in terms of the small unit, because it

1 We can start by defining 10^8 lines of force as the unit of induction and call it, say a weber. A micro-weber would be then a hundred lines of force; 200 micro-webers per square centimetre would be what we call to day 20,000 lines of force per square centimetre Webers of induction multiplied by number of turns, multiplied by number of revolutions per second, multiplied by a numerical constant would give us then the electromotive force in volts.

Some such arrangement would preserve the harmony in the system of our units and it wou'd also give a very convenient value to the new units.

is then apparent whether or not any factors go out of our equations by cancellation.

PROF. CROCKER:—I do not think it is the idea of the committee or of any of its members, or for that matter, of any member of the Institute, that we can succeed in disposing of a question of this sort in one evening. It was the intention merely to bring it before the Institute so that its members could have time to consider it and discuss it generally in order to get the matter started, and then let it soak for a while, so to speak, and be taken up at some future meeting. I think, so far, the discussion has been very satisfactory, and I believe a little later in the year, we can take it up for final action.

THE CHAIRMAN:—I think there is no doubt that Mr. Crocker is right. It is quite too much to expect that we can settle any of these things in an evening.

[Adjourned.]

Report of the Delegation to the Frankfort Electrical Congress.

To the Council of the American Institute of Electrical Engineers.

Gentlemen:—As chairman of your delegation to the International Congress of Electricians, held at Frankfort o. M., Germany, September 7-12, 1891, I beg leave to submit the following report.

Of the members of your delegation the following were present during the whole session of the Congress, viz: Mr. L. Gutmann, Prof. Heinrich, Mr. Carl Hering and Prof. Nichols. The only member not present was Mr. Tesla, whose unavoidable absence is to be regretted very much.

Your delegates had received no definite directions regarding matters to be brought before the congress, the choice of which was therefore left entirely to them. They concluded to bring up two or three matters which had occupied the attention of the Institute, namely the adoption of the name "henry" and a normal value for the resistance of copper and in addition suggest the adoption of a practical unit of magnetism.

The substance of our propositions as duly announced on the printed programmes was as follows:

1. That the American Institute of Electrical Engineers had adopted the same "henry" for the name of the practical unit of induction and that we propose that this congress formally adopt the name. This was followed by an explanation of the value of this unit, why this name was preferred to the word "quadrant," and why it was named after Prof. Henry.

2. That this congress ought to adopt a name and value for a practical unit of the intensity of magnetism. To open a discussion we proposed the name "gauss" and a value of 1,000 lines per square centimetre, or 10^3 absolute units.

3. That it would be desirable to adopt a normal value for the resistance of copper to facilitate relative comparisons for commercial purposes. To this we added that we wished merely to agitate this question here in order that any preliminary tests to determine such a value may be made between now and the next congress, so as to facilitate its adoption at that congress.

4. The announcement closed with an invitation to the members of this congress, on the part of the American Institute of Electrical Engineers, to participate in the next International Con-

gress to be held under the auspices of this Institute, in Chicago, in 1893 (at that time your delegates were not informed that this coming congress was in the hands of a committee of the World's Fair).

There were other matters which your delegates might have added, but it was thought best under the circumstances to limit ourselves to only the most important.

Seeing that there were decided differences of opinion among some of the prominent foreign members of the congress, as to whether the most important magnetic unit to be named was the unit of intensity or one of quantity (flux), the chairman of the delegation added the suggestion that both units be named and proposed the name "weber" for the latter.

When the consideration of our proposition came up at the general session, it was referred—together with the proposition of Mr. Hospitalier to establish a uniform international system of notation and conventional signs and symbols—to a special committee, to report at the next general session. This committee was appointed by the chair and consisted of Prof. Ferraris, Chairman; Mr. Hering, Secretary; and a list of members of which the following were present at the meetings: Braun, Grawinkel, W. Kohlrausch, Loewenhertz, Strecker, Uppenborn, (Germany); Ayrton, Preece, Silvanus P. Thompson, (England); Ferraris, Roiti, (Italy); Hering, Nichols, (United States); Hospitalier (France); Weber (Switzerland).

The programme of the congress provided for almost every hour, so that there was only very little time left (two meetings of an hour each) for the meetings of this committee. Mr. Hospitalier's proposition occupied almost the whole time at these meetings, leaving only a very short time for the consideration of our propositions. The English members appeared to favor the adoption of the name "henry," Mr. Preece saying that he had received instructions from the Royal Society to advocate its adoption. The German, French and Swiss members opposed it on various grounds. Some thought it conflicted with the already adopted name, "quadrant;" others thought it ought to be "Franklin;" others "Neumann," and some did not acknowledge the work of Prof. Henry in this field. Quite a number had never heard of its adoption by this Institute. The names "gauss" and "weber" for the magnetic units, appeared to meet with general approval. The disagreement was as to what their numerical values

should be. It appeared to be preferred to make them both 10^8 absolute units. The matter of the normal resistance of copper did not come up at all for want of time.

It was suggested that these subjects were too important to be considered hastily during the few minutes which were left for this meeting. It was thought better not to do anything than to do something which might afterwards be regretted. For these reasons and seeing that this committee, (for want of time for preparation) was entirely unprepared to discuss the subjects, our propositions were withdrawn. Had there been sufficient time for proper consideration, I think there is little doubt that our propositions would all have been adopted, except that of the normal resistance for copper, which number the Germans have no use for, because they usually express the quality of copper by the conductivity of a unit of length and cross-section.

Although our propositions were not adopted, chiefly on account of lack of time, the effect of our making them has prepared the way for their adoption at the next congress. I would suggest that the Institute profit by this experience, and that, as the leading American electrical society, it ought to take the lead in the matter of establishing and naming practical magnetic units and other similar international matters, by considering the subject now at their meetings, making propositions, publishing them in domestic and foreign journals, inviting the aid and criticism of foreign electricians and societies, and in this way prepare for their final settlement at the next congress. A congress is hardly the place to start a discussion on such matters, it is the place to finally settle a discussion which should precede it in the societies and journals of the leading nations.

In this connection I would call the attention of the Institute to the important matter of a universal and international system of notation and electrical symbols. This subject was very ably discussed in a paper read before the congress by Mr. Hospitalier, who has for years given the subject much attention and has formulated a complete system, parts of which were adopted at this congress. The committee named above were instructed by the congress to discuss the subject between now and the next congress and bring it to the notice of the public through the journals, so as to facilitate the adoption of some such system at the next congress. I would suggest that this Institute take up the matter in this country and that it take a leading part in

soliciting opinions and criticisms from others; also that this be done as soon as possible, as the question of an international system will not be an easy one to settle.

It is in place here to call the attention of this Institute, as a promoter of the next international congress, to one of the faults of this congress. There appears to have been no preparation made for international business; besides the social part of the programme which was very creditably arranged and was highly enjoyable, there was nothing else but the reading of papers, and of these a number were of such a character that it would have been better to have read them by title, or at least, at special meetings designed for such papers. It would have been better if all papers had been passed upon, beforehand, by a competent committee and classed by them, preference being given in time and place on the programme in accordance with their merits, and general interest. A congress which people visit from all parts of the world, is hardly the place to spend much time in a lengthy description of a new piece of apparatus, a proposed invention, priority of inventions, etc.

In conclusion, I wish to add that the high standing of the Institute in Germany was shown by the great kindness and courtesy extended to your delegates There was nothing left undone by the managing committee to show their appreciation of the high standing of the Institute and to recognize the part it took by sending a delegation to the congress. The Institute will receive a copy of the proceedings when published. It was also presented with a photograph of the original of a very interesting letter of the year 1830, by Gauss, describing what was one of the first electric telegraphs in actual use.

Respectfully yours,

CARL HERING,

Chairman of Delegation.

Philadelphia, October, 1891.

A paper read at the 61st Meeting of the American Institute of Electrical Engineers, New York, Nov. 24th, 1891. Vice-President Lockwood in the Chair.

NOTES ON THE FRANKFORT ELECTRICAL EXHIBITION.

BY CARL HERING.

The list of the international electrical exhibitions which have been held so far, begins with the one in Paris in 1881; this was followed by the one in Munich in 1882, then in Vienna in 1883, and Philadelphia in 1884, which was the last of this annual series of exhibitions. After a lapse of five years, during which time there were none of importance, it was followed by the Universal Exhibition in Paris in 1889; although this was not an electrical exhibition, the electrical exhibits formed such a large and important part that they may well be said to have formed the equivalent of an electrical exhibition. This was followed by the one at Frankfort, held this year, which forms the sixth and last, and which is to be followed by the electrical section of the World's Fair in 1893, making the seventh. Among the electrical exhibits made at exhibitions prior to the Paris Exposition of 1881 may be noted the first exhibit of the telephone, and a few arc lights run with a Gramme dynamo, both at the Centennial Exhibition in Philadelphia, in 1876; also a very small electrical exhibit, chiefly of telegraph instruments and apparatus, at the Paris Universal Exhibition of 1878.

The most prominent features at the electrical exhibitions were as follows: In the first one at Paris, in 1881, the chief feature was what might be called the birth of the present electrical industry. The most prominent exhibits included large dynamos of proportions thus far unknown; practical systems of distribution of arc lamps in series, and incandescent lamps in multiple arc, and practical systems of telephonic distribution. In Munich,

the most prominent feature was the electrical transmission of power, about which so much was written, though so little was accomplished. In Vienna, in 1883, and in Philadelphia, in 1884, there were no particularly prominent features, electrical engineering having by that time developed into a steadily growing industry, as distinguished from one developing with spasmodic strides as in the previous years. In Paris, in 1889, the prominent feature ought to have been alternating current distribution, but owing probably to the absence of certain American and Austrian exhibitors, such systems were not as prominent there as their importance and their development at that time would have warranted. The alternating current apparatus of Professor Thomson and his welding machinery were unquestionably the most interesting novel features at this exhibition. At the Frankfort Exhibition, the most prominent feature was very high tension alternating current transmission, and alternating current motors. What the World's Fair has in store for us is to be seen, but electrical traction will doubtless be among the most prominent of the later developments in the electrical industry.

The two exhibitions in Paris were truly international in character; but in the others, except perhaps in the one at Vienna, there were so few exhibits from foreign nations that they can hardly claim to have been more than national exhibitions.

International electrical congresses were held in Paris in 1881 and 1889, in Philadelphia in 1884, and in Frankfort in 1891; of these, the first did the very creditable work of laying down the admirable system of electrical units and names which stands today as the most simple, rational and uniform system of complex units of measurement existing in any branch of engineering. The third congress, in 1889, also did good work in the field. The one in Philadelphia in 1884, and in Frankfort this year, accomplished practically no international work, leaving all the more to be done at the fifth congress, to be held in Chicago, in 1893.

The Frankfort Electrical Exhibition was started, I understand, by the electrical society of that city; it was conducted by some of the leading business men and electricians of that city, and, I believe, with some assistance or recognition from the government. It was therefore more of a private rather than a national enterprise. In Frankfort the foreign exhibits were limited to a very few, chiefly from Switzerland, England and the United States; it can not, therefore, lay much claim to having been international;

it certainly was not, so far as representing the state of the art in other countries was concerned. It was an excellent exhibition of the state of the industry in Germany, but that was all.

The general arrangement of the grounds, buildings and exhibits was very creditable. The grounds and most of the buildings were attractively arranged, with a view to giving it a pleasing appearance and making it an attractive resort for the public, with as many "side shows" as possible, for which, however, an extra admission was charged. The average price of admission being only 18 cents, and on Sundays 5 cents, it was visited largely by persons who came there for the amusement, more than for studying the exhibits. I mention this because without these attractive features it could not have been a financial success. The number of visitors during the six months was somewhat over a million. The attraction afforded by some of the "side shows" may be judged from the fact that of the visitors at the exhibition, one out of every three visited the panorama, a painting of a transatlantic steamer entering New York Harbor; the only electrical feature about it was that in the evening it was lighted by electricity.

Among the popular attractions on the ground, may be mentioned the following: A moderate-sized theatre with electric illumination from accumulators, and a very complete system for manipulating the lights to produce stage effects. Popular and very fully illustrated lectures were given here every afternoon, while in the evening there was a performance consisting of a pantomime or ballet, in which the plot was a senseless, though otherwise amusing mixture of mythology and of the history of electricity from the frog experiments to the crowning glories of the electric light. In a miniature model theatre, the firm of Siemens and Halske exhibited a very complete system of stage illumination, capable of producing very varied and really beautiful stage effects. This exhibit was a very successful one, and leaves no doubt that electric light is not only the best, but also the only means for producing such fine illumination effects on the stage. Many of these effects could not have been produced with gas or calcium lights, not to speak of the danger of fire attending such an attempt. The imitation of the sunset, the alpine glow and the rising of the moon, were particularly fine and in some respects fully equalled those of nature.

On the grounds was an artificial waterfall pumped electrically

from the river, with both direct and alternating current motors, and illuminated internally and externally with hidden colored electric lights. The internal illumination was not a success, as had been demonstrated before, both at Paris in 1889, and at Philadelphia in 1884, but the external illumination was quite fine and attractive, though hardly comparable with the really beautiful effects produced in the illuminated fountains in Paris in 1889, by an English firm. The total illumination of the grounds and buildings was somewhat over a million candles. In the telephone building there was a long-distance transmission from the Opera House at Munich, a distance of about 200 miles. Male and female voices could be distinguished from each other, and sometimes even the different instruments of the orchestra. Besides this long-distance transmission, there were also several others from music halls and theatres in the environs, in which the transmissions were very good.

Among the minor popular attractions were the following: A diver working under water by electric light; a sort of artificial race course for children, in which each horse was operated by a separate motor under the track, controlled from a common platform; an artificial mine, underground, with mining drills, pumps, ventilators and a small railroad; electric elevators, railroads and launches, etc; a boat with a benzine engine, both made entirely of aluminium produced by an electric process. One of the attractions greatly appreciated by the public was not electrical at all, being a series of workshops in active operation, for the manufacture of goods of all kinds, in which the only electric feature was that the machinery was driven by electric motors.

These popular exhibits, together with two typical German attractions, viz., excellent music and the numerous "Bier Hallen" formed the chief features for the public. On the other hand, the exhibition was by no means without attractions for the electrical engineer; the exhibits which interested him, far outnumbered those for the public. It may be said, however, that with the exception of a few exhibits, there was little shown which was not already known to the engineer through the foreign and domestic electrical journals. In these days of enterprising journalism, there is probably little of novelty in the electrical industry which does not find its way into the journals. The electrical journals of to-day may be said to form our permanent

electrical exhibition. Although most of the exhibits were already known to the engineer, through their published description, it was nevertheless very interesting to see them, and examine them in operation. It was in this feature that the exhibition was most interesting to the engineer, and for this reason a description, such as could be given here, is a poor substitute for a visit to the exhibition itself. I will therefore confine myself here, merely to some general statements, leaving detailed descriptions to the journals, where they more properly belong and where many of them have already appeared.

In general, and with but few exceptions, there was one very noticeable feature to an American about the exhibits, and one that is to a certain extent characteristic of the two nations. The Germans are very careful, thorough, indefatigable workers; their work shows thought, intelligence and a thorough knowledge of the underlying principles (though not necessarily knowledge of what has been done outside of their own country), but they appear to lack invention. They will work out with much trouble and care, the best theoretical conditions (which sometimes lead them to impracticable constructions), but they lack the faculty of invention which suggests new practical conditions and simple ingenious contrivances. The Americans, on the other hand, possess the faculty of invention to a great degree, but their inventions and ideas, although bright and ingenious, not infrequently show the want of a fundamental and thorough knowledge of the subject, or a want of study and care in carrying them out. The numerous rejected or absurd applications in our patent office will show this. These statements are, however only quite general, there are of course many notable exceptions in both cases. In general, therefore, it may be said that there was a noticeable absence of novelties among the exhibits at this exhibition, but, on the other hand, the exhibits showed careful, intelligent and very creditable work.

There was another notable feature which has its parallel, to a certain extent, in the watch industry. In America the electrical industry has developed to such a degree that dynamos and motors are made from the same templates, and probably often with interchangeable parts. They are kept in stock, and installations must be made to suit these standard sizes. I am told that in one of our largest factories, anything different from the regular standard sizes or designs is undertaken with great reluctance and

little interest. In Germany, on the other hand, the dynamos, I am told, are as a rule made to suit the installations, and are varied in design to suit the requirements. In numerous cases in which I wished to find out what their usual practice was, it turned out that there was no usual practice; that the exhibit was the first, or among the first of its kind made. In a pattern shop of one of the largest companies, I noticed about six different patterns for very large dynamos all for about the same output, which had been used for only a few castings, and some for only one. Yet they state that they make a profit, even though the dynamos are very much cheaper than in the United States, while they are often fully as good, if not better. While the one system is more flexible and undoubtedly leads to a more perfect development of the dynamo, the other, although it does to a certain extent retard development, is unquestionably the most practical method when the sales are large, and it will no doubt be employed there when the industry assumes the proportions it has assumed here.

There was still another feature which was noticed, and I mention it here, not with mere fault-finding intentions, but with the hope that we may correct a fault which we must admit does exist, to a certain extent, at least. In America there is sometimes a tendency to sacrifice quality of work for quantity, which is a natural result of the objectionable practice of awarding a contract to the lowest bidder, and it is, therefore not necessarily a fault of the manufacturers. In Germany, much pride is taken in making and keeping one's reputation for doing good work; so much so, that the name of a company and the quality of the work they have done has perhaps more weight in awarding a contract than the price. The natural result is that the work is more creditably and carefully done; there is a greater variety in the systems used, and they are better adapted to suit the special requirements of the particular installations. A noticeable feature to show the preference for substantial work is seen in one of the accumulators exhibited. Here in America the tendency has been to get the greatest possible output per pound of weight, or in other words to put in as little material as possible; the result is that the accumulator here is a frail, delicate object which must be accompanied by a nurse, or its life will not be guaranteed by its maker. In Germany they have discarded this policy to a great extent. The accumulator which is used there most frequently, and much more in proportion than accumulators are used here,

is a strong, heavy, hardy, Planté cell, with plates so thick that they are able to stand without being watched so carefully. The result of this is, that there are many installations and central stations there equipped with accumulators. I call attention to this particular case, as there is no doubt that in the application of storage batteries to central stations they are far ahead of us, and we can learn much from them.

Regarding the nature of the recent progress in the electrical industry as exhibited there, it may be said that, excepting the rotary current systems, there was no radically new departure shown in any of the prominent branches. The exhibits, as a rule, showed that the erratic progress made in the earlier years, was being replaced by a steady, natural and healthy progress, such as may be expected in any important industry, receiving the attention that this one does. Impracticable and absurd ideas are dying a natural death, and the wholesome effects of the law of the survival of the fittest are plainly to be seen. Although the progress of the introduction of electric light and power in Germany, has been much slower than in the United States, it has been, and continues to be a steady progress; the Germans may think twice before they act, but when they do act they do their work well and thoroughly. New York was the first city to have a large local central station plant for incandescent lights in a small limited district, but Berlin will soon be ahead of New York if it is not so already, in having an extended, homogeneous, well-planned, underground system for supplying incandescent lights to a very large part of the city. The motive there is not to pay the greatest dividends, to speculate with franchises, or to supply politicians with boodle; their leading object is to supply the inhabitants with a reliable system of lighting, as cheap as is consistent with thoroughness. It is needless to say that in a well-regulated government like that of Germany, the government controls the companies, and does not allow itself to be controlled by them. In street lighting, and doubtless also in the distribution of power and the application of motors, they are far behind us, and can learn much from us, but in their manner of introducing complete systems of supplying light in their cities, we have much to learn from them.

Electric railroads on the continent were conspicuous by their almost total absence; yet the conduit system used in Budapest, in Hungary, which is probably the largest network of lines in

Europe, is apparently one of the most satisfactory of any systems in use anywhere, here or abroad, at least where the appearance of the streets has any weight at all. One has only to compare a certain place in Budapest, where there are many crossings, curves and switches, with a similar place in Boston, to appreciate the difference from an æsthetic standpoint. The trolley wire is no doubt the best system for suburban and interurban lines, or for small towns; but in a large city, which takes any pride in its appearance, the trolley wire should and will give way to less hideous systems.

Proceeding now to the individual exhibits, it is not the place here to attempt to describe them in detail. I will confine myself here to mere general remarks, leaving detailed descriptions to the journals where they more properly belong.

The historical exhibits were quite naturally limited to those of Germans. Great prominence was given to Soemmering, as the original inventor of the telegraph in 1809. His statue was seen in several places, and his original apparatus was exhibited in operation. The original apparatus of Reiss, the inventor of the telephone, the electric machine of Goethe, and the original apparatus of Hertz, were among the most interesting historical exhibits.

Among the many firms exhibiting, the most prominent was, no doubt, the firm of Siemens and Halske, the largest electrical manufacturers in Germany. Their field is apparently unlimited, as they exhibited creditably in almost all the branches of the electrical industry. They appear to make a specialty of nearly every branch, and in some of them they unquestionably excelled. The firm of Schukert had the next largest exhibit; although not so broad in scope, it was in some respects possibly more creditable. "Helios," representing the Austrian firm of Ganz and Co., came next in order of size of their exhibit, which was limited entirely to alternating current machines and systems. The exhibits of these three firms, one at each end and one in the middle, occupied the greater half of the machinery hall. The Allgemeine Electricitæts Gesellschaft, of Berlin, and the Oerlikon Co., of Zurich, came next in point of prominence. Their joint exhibit of the high tension rotary current power transmission may be said to have excelled the others in point of interest, as it was undoubtedly the most interesting exhibit to the engineer, as well as the most novel.

The exhibition of dynamos were, as a rule, very creditable, both as to workmanship, and particularly as to absence of sparking at the brushes. It can not be said that any one form predominated; there was, in fact, probably a greater variety of form than at any other exhibition; there were scarcely two alike. A very noticeable feature was that all large machines were multipolar; large bipolar machines apparently do not exist in Germany, and in this respect they are doubtless ahead of us. One reason for this is, no doubt, that their large dynamos are almost without exception coupled directly to the engine. This leads to slow speeds, and consequently to armatures of large diameters, which, of course, excludes bipolar fields. In several prominent cases the armatures were outside of the radial multipolar fields, a practice which, I believe, is not attempted in this country at all. A happy medium between the two, appeared in the large flat ring armature of Schukert, which combines the advantages of both. The largest dynamo was that of Siemens and Halske, having ten radial poles, around the outside of which revolves a large Gramme ring armature, ten feet in diameter, whose external surface is bare and acts as the commutator. There are ten sets of brushes of three each, making thirty brushes to take care of. These dynamos have an output of 600 kilowatts at 100 revolutions, and are the units for their central stations, chiefly for those in Berlin. They state that about thirty of these have been installed. The large alternating current dynamos exhibited were made according to the old models, with radial magnets. A very serious objection to this form appears to be that the machines make a very loud humming noise, like a very low-tuned steam whistle. So loud was this noise that it made conversation difficult, and, in fact, almost impossible in the neighborhood of the large machine, for which reason it was called the "growler" of "grumbler" by the rival exhibitors. Among the more unusual forms of dynamos may be noted the Fritsche continuous current, and the Kingdon alternator, having no moving wire, both of which have been described in the electrical journals.

It appears that carbon brushes are limited chiefly to motors, and especially to railway motors, where they appear to have replaced the others entirely. The form of brush almost exclusively used for dynamos is one made of fine copper wire gauze; an important feature being that the fibres run diagonally and not crosswise, thereby avoiding a fraying out at the ends, which was

formerly the great objection to them. The chief advantages of this brush are, that it is soft and does not "scream" like wire or foil brushes, and that it is easily made; it is strange that it is not adopted in this country. Bearings are, as a rule, made with self-feeding arrangements, like a ring or a wick having an oil reservoir below the bearing. Speeds are almost invariably low, and armatures large. Field magnets are generally very short and thick, a natural accompaniment of multipolar fields. Large dynamos are, as a rule, coupled directly to the engine. The workmanship on dynamos was, as a rule, very good, as was also the construction, apart from the design; the design was in a number of cases awkward and cumbersome.

Among the various systems of distribution exhibited, the simple 110-volt continuous current system still leads for isolated plants. Alternating current systems are not used for smaller plants, being chiefly limited to central stations. Arc lamps are, as a rule, run from the same mains as the incandescent, being connected two in series, together with a dead resistance. This resistance represents a dead loss of power, reaching as high as 20 and 25 per cent., but it is a necessary part of such a system, as the lamps often have shunt coils, which would be of no use as a source of regulation, if there were no resistance in series with the lamp. Whatever disadvantages attend such an arrangement, it must be said that the arc lamps burn far better there than here. Our restless, flickering lamps contrast very unfavorably with the average lamps seen at this, or at the late exhibition at Paris.

Although the amount of power represented by the alternating current systems was quite great, the number of exhibits of this system were comparatively small, as compared to the others, showing that the continuous current systems still predominate in Germany. The largest dynamos of the alternating type were those of the "Helios" Company (Ganz and Co.), and of the firm of Siemens and Halske. The "Helios" Company uses 2,000 volts and about 83 alternations; arc lamps and small motors were run indiscriminately from their mains. The next most important system of distribution was the accumulator system, in connection with the three-wire distribution, an arrangement which I believe is seldom used here, though it seems to be quite common there, judging from the exhibits. In this system one or more dynamos of about 250 volts charge two sets of accumulators in series, each of about 125 volts. These are con-

nected to the three mains of the three-wire system, the neutral wire being connected to the middle of the series. By means of switches connected to the outside cells, the voltage for the long and the short feeders may be made greater and less, respectively, and the cells may be cut out when fully charged.

The only important series system for arc lamps was an American exhibit by the Thomson-Houston Company. Such systems may truly be called American, as they do not appear to be used to any extent by the German or French companies, who seem to acknowledge that we are their superiors in this branch. The result of the neglect to develop this system is, that in European cities arc lamps are rarely seen in the streets, at least when we compare their streets to those of American cities.

The series incandescent system, I believe, was not represented at all at this exhibition. It also may be called a truly American system. The five-wire system was represented, but it failed to leave a favorable impression, and, as far as I could ascertain, it is in use in a central station in only one city, the home of the company advocating the system. This fact speaks for itself.

Among the systems of distribution of power, may be mentioned above all, the rotary field system of the Lauffen-Frankfort exhibit. It is truly a system of distribution as compared to a mere system of transmission. Next in interest were the alternating current motors, run from the "Helios" Company's system. They were mere alternating current machines, with a rectified current in the field, obtained from a commutator. They appear to have a dead point, at which they will not start. They make some noise at starting, and there are more or less brilliant arcs at the commutator until they run synchronously, but after that they appear to run well. Motors from 1 to 5 H. P. will start in from 6 to 28 seconds.

Among the systems of simple transmission of power, as distinguished from systems of distribution, the largest was a 2,000 volt continuous current system exhibited by Lahmeyer. The receiver in this system was an ingenious continuous current transformer of the motor dynamo type. The system of switches for starting, controlling and regulating the power, and for protecting the apparatus as well as the attendants, was very complete and very creditable to the designer, but the whole system failed to show any material advantages over the alternating current systems; on the contrary, it left the impression that it was

inferior. Besides these systems there were also a few motors run with a two-phase rotary field current using four wires, by Schuckert. These motors were started unloaded, and without a field current; the latter, obtained from a commutator, was switched on when the motor had reached synchronism.

The exhibits of electric railroads were meagre, and, with one exception (the Budapest system), they were unsatisfactory to an American, as they were experimental systems, as distinguished from normal systems. In this branch they have much to learn from us.

Among the transformers, there was apparently nothing of particular interest. The firm of "Helios" have abandoned the old form of Zipernowsky-Deri (for Faraday) transformers, and have adopted one like the usual forms, in which the disposition of the coils is a more practical one. Motor dynamo transformers, or dynamotors, were quite frequently seen, and appeared to be used very much more frequently than here.

NOTE.—The remainder of the paper was a verbal description of some of the more prominent exhibits, accompanied by slides showing a number of interior and exterior views of the grounds and buildings, and detailed views of some of the exhibits, more particularly of the Lauffen-Frankfort transmission experiment, most of which have been published in the foreign and domestic journals, and are therefore omitted here at the suggestion of the author.—EDITOR.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, December 16th, 1891.

The sixty-second meeting of the American Institute of Electrical Engineers was held this date at No. 12 West 31st street. The meeting was called to order by Secretary R. W. Pope.

THE SECRETARY:—Gentlemen, it gives me pleasure to announce that we have with us this evening our esteemed Past President, Professor Elihu Thomson, and in view of the fact that he is not very frequently present, we have induced him to preside over the meeting to-night, but it is understood that he will not refrain from taking part in the discussion should occasion warrant.

THE CHAIRMAN [Prof. Thomson]:—On being asked to preside this evening, I protested that I did not believe I was a Vice-President of the Institute, but the plan seems to be that I shall make up to some extent for my remissness in the past. When I was President of the Institute I did not get down to many meetings, except the annual meetings, and I will take pleasure in doing what I can now to make up for that remissness, if my services are of any use to the Institute.

The proceedings of the evening will commence with the paper "On Polyphasal Generators," by Dr. M. I. Pupin, of Columbia College.

A paper read at the sixty-second meeting of the American Institute of Electrical Engineers, New York, December 16th, 1891. Past-President Thomson in the Chair.

ON POLYPHASAL GENERATORS.

BY M. I. PUPIN, PH. D., COLUMBIA COLLEGE.

Few will deny the importance of the polyphasal current systems; none the fascination of their study. This belief induced me to present the following brief essay before the Institute.

The experimental researches in this new and promising field of electrotechnics are not yet numerous, but still the results already obtained are of so decisive a character as to leave no doubt whatever as to the extremely high practical importance which is attached to electrical generators, motors and transformers constructed according to requirements imposed upon us by this *new method of combining a set of variable electromotive forces.* For who among us does not thoroughly appreciate the beautiful inventions of Nikola Tesla and the completeness of the success which Dobrowolsky and Brown obtained by the practical applications of these inventions?

The exact quantitative relations involved in the polyphasal system of currents are not, I venture to say, quite as well known as its practical results. To give an impulse to further inquiry in that direction is one of the principal aims of this modest investigation. For the present I propose to confine myself to the polyphasal generators in general, and particularly to *polyphasal generators whose system of electromotive forces is capable of producing a rotary magnetic field of constant strength.* The last point seems to me to be one of the vital points in this new method of electrical distribution. It is in this particular point that Mr. Dobrowolsky claims his system to be superior to that of Nikola Tesla.

Let us consider the theoretically simplest form of a polyphasal

generator, as shown in Fig. 1. A non-magnetizable ring with n open equal coils at equal distances from each other rotates uniformly through a perfectly homogeneous magnetic field Let PP′ be the neutral plane of the field. At the instant when coil 1 is at the angular distance θ from the neutral plane PP′ the E. M. F. generated in the various coils will be

$$e_1 = K \sin (\theta + a)$$

$$e_2 = K \sin (\theta + a + \frac{2\pi}{n})$$

. . . . . .

. . . . . .

$$e_n = K \sin \left\{ \theta + a + (n - 1) \frac{2\pi}{n} \right\}$$

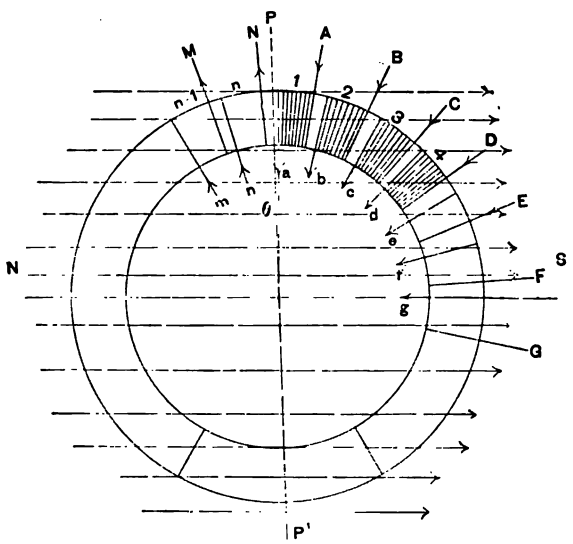


Fig. 1.

Where K is a constant depending, as is well known, on the field intensity, the speed of rotation, the number of turns in the coil and the area of the plane of a turn ; a is the angular width of one-half of the coil.

Since

$$\sin(\theta+a)+\sin (\theta+a+\frac{2\pi}{n})+\ldots\ldots+\sin \left\{ \theta+a+(n-1)\frac{2\pi}{n} \right\} =0$$

it follows that

$$e_1 + e_2 + e_3 + \ldots\ldots + e_n = 0 \ldots\ldots (1).$$

That is to say, *the sum of electromotive forces generated in the various coils which are on one side of the neutral plane is numerically equal and of opposite sign to that of the coils on the other side of this plane.* This result is well known and self-evi dent. It is, however, far from self-evident that relation (1), which I shall call the *relation of continuity* for the electromotive forces, will be satisfied by every magnetic field.

Close each coil separately by conductors of equal resistance and self-induction. Let $c_1, c_2 \ldots\ldots c_n$ denote the currents in the n separate circuits. It is evident that

$$c_1 = \frac{K}{I} \sin (\theta + \alpha - \varphi)$$

$$c_2 = \frac{K}{I} \sin (\theta + \alpha + \frac{2\pi}{n} - \varphi)$$

$$c_3 = \frac{K}{I} \sin (\theta + \alpha + 2\frac{2\pi}{n} - \varphi)$$

$$c_n = \frac{K}{I} \sin \left\{ \theta + \alpha + (n-1)\frac{2\pi}{n} - \varphi \right\}$$

Where I is the impedance in each circuit and φ the angle of retardation. Hence, we have

$$c_1 + c_2 + c_3 + \ldots\ldots + c_n = 0 \ldots\ldots (2).$$

That is to say, *the relation of continuity is satisfied for the currents also.*

Let the wires $aA, bB, \ldots\ldots nN$ (Fig. 2) represent a part of each of the n conductors of this system. Then, according to relation (2), the sum of the currents in these n linear conductors being always zero, if we joined them all into one conductor there would be no current in this wire, but the currents in the n circuits would circulate exactly the same as before. In fact, the common juncture is useless and can and should be cut out.

The diagram, Fig. 3, represents this method of connecting for a three-phase system. Consider, now, n equal coils distributed at angular distances of $\frac{2\pi}{n}$ over a laminated iron ring B, each coil being a part of the n conductors coming from the generator. Diagram Fig. 4 illustrates this for a three-phase system. Let the n currents be denoted now by $c_1', c_2', \ldots\ldots c_n'$. We shall have, now,

$$c_1' = \frac{K}{I'} \sin(\theta + a - \varphi')$$

$$c_2' = \frac{K}{I'} \sin\left(\theta + a + \frac{2\pi}{n} - \varphi'\right)$$

$$c_n' = \frac{K}{I'} \sin\left\{ \theta + a + (n-1)\frac{2\pi}{n} - \varphi' \right\}$$

and therefore

$$c_1' + c_2' + \ldots\ldots + c_n'' = 0 \qquad (3)$$

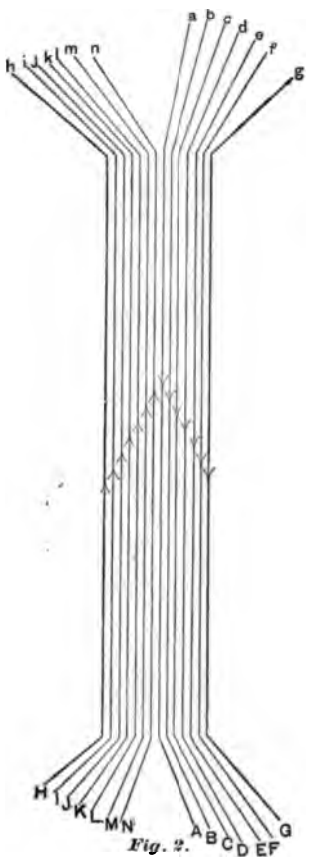


Fig. 2.

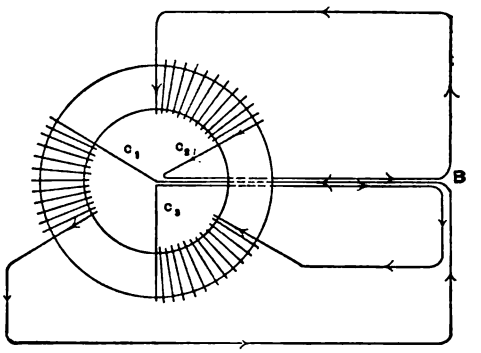


Fig. 3.

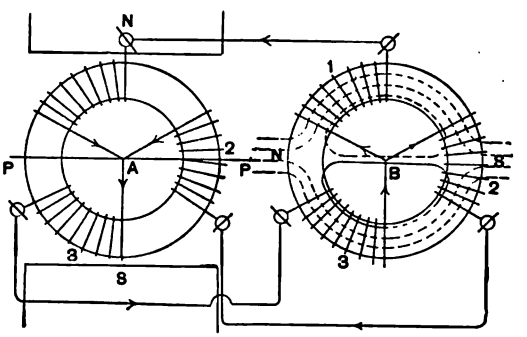


Fig. 4.

The introduction of the iron ring with the n coils into the n phasal system has changed the impedance I, and the angle of retardation φ; but this change is evidently the same for all coils. The correctness of this statement might, perhaps, be questioned, if we supposed that the system of the n variable currents was at

any moment strong enough to saturate the iron ring, I therefore suppose that the intensity of magnetization in the ring is never over 10,000 C. G. S. lines of force. We shall presently see that in the case of a properly built generator the saturation of the iron ring will not vitiate the correctness of the above statement in the slightest.

Let s be the number of turns in each of the n coils. Relation (3) gives

$$4\pi\, s\, c_1' + 4\pi\, s\, c_2' + \ldots\ldots + 4\pi\, s\, c_n' = 0 \qquad (4)$$

That is to say the magneto-motive forces in the n coils of the ring b satisfy the relation of continuity. Relation (4) translated into physical language means that the *magnetization in the iron ring is due to two equal magneto-motive forces working in multiple arc. The magnetic field produced* is *perfectly symmetrical with respect to the ring as indicated by the dotted lines in Fig.* 4.[1]

Consider now n iron ring cores of exactly the same dimensions and made of the same material. Let ρ be the reluctance of each ring. Let each of the n coils be interlinked with one of the iron rings, we shall have n homogeneous magnetic circuits; and as long as the magnetization of these rings is considerably below the saturation point, we shall have

$$\frac{4\pi\, s\, c_1'}{\rho} + \frac{4\pi\, s\, c_2'}{\rho} + \ldots\ldots\ldots + \frac{4\pi\, s\, c_n'}{\rho} = 0$$

That is to say the magnetic induction in the n magnetic circuits obeys the same law as the n electric currents; we can therefore employ the method of polyphasal connection for the magnetic circuits also and we obtain what the Germans call a Verkettung der Magnetischen Kreisläufe which may be translated into English by a more accurate expression: *Polyphasal coupling of magnetic circuits.* A transformer constructed on this principle may be called a *coupling transformer*, to distinguish it from the Tesla polyphasal transformer.

A simple consideration will show that the field rotates around the axis of the ring B synchronously with the rotation in the generator which produces the impressed E. M. forces. Consider the armature of the generator. Since the ampere turns on one side of

(1) This will be strictly true when the number of coils over the ring B is even, because then the distribution of the ampere turns over the ring is perfectly symmetrical. It is therefore always strictly true because the number of these coils may be made even in odd number of phases as well as in even number of phases.

the neutral plane is always equal and opposite in sign to the ampere turns on the other side of this plane it is evident that the magnetic field due to the ampere turns in the armature is fixed in space and perfectly symmetrical with respect to the plane of symmetry PP[1]. We can therefore say that this field, though *fixed in space, rotates with respect to the armature* with the *same angular* velocity with which the armature rotates in space. The distribution of the ampere turns over the stationary ring B being at any moment the same as that over the armature ring, it follows that the magnetic field of B also rotates with respect to B synchronously with the rotation in the armature. An inspection of the diagram in Fig. 4 will show that *when the rotation in the generator is reversed the rotation of the field* B *will also be reversed.*

The strength of the rotating magnetic field will vary because the strength of the two equal magneto-motive forces which are working in multiple arc will vary. The following simple consideration will show us the law of this variation. Two cases must be considered separately. First, when n is an odd number; secondly, when n is an even number.

CASE 1.

A simple definition will save me tedious repetitions of long sentences. I define the sum of all the electromotive forces generated in all the turns which are at any moment on the same side of the neutral plane of the generator as the *resultant impressed* E. M. F. at that moment. The magneto-motive force of the rotating field will evidently vary according to the same law as the resultant impressed E. M. F. To find the law of variation of the resultant impressed E. M. F., consider the armature of the generator when the angle θ of coil 1 is zero. To make the reasoning shorter, I make now the angular width of each coil equal to $\frac{2\pi}{n}$, so that the n coils completely cover the ring, which makes $a = \frac{\pi}{n}$. If this angular width is smaller, then a simple consideration will show that the law of variation which I am about to deduce will be exactly the same. In the position just mentioned, the coils 1, 2, 3, ... $\frac{n-1}{2}$ will all be on the same

side of the neutral plane, whereas coil $\frac{n+1}{2}$ will be just half on one side and half on the other side of this plane. There is no electromotive force generated in this coil. As the above mentioned angle θ begins to increase from zero, coil $\frac{n+1}{2}$ begins to contribute to the resultant impressed E.M.F., but this contribution is just counterbalanced by the loss due to the entrance of coil n into the opposite region of the neutral plane. The variation in the resultant impressed E. M. F. is therefore due solely to the change of position of the turns in the coils 1, 2, 3, ... $\frac{n-1}{2}$, on one side and the corresponding turns on the other side of the neutral plane. This will be the case until coil $\frac{n+1}{2}$ has completely passed to one side of the neutral plane and coil n is just bisected by it. During this interval θ has increased from zero to $\frac{1}{2}\ \frac{2\pi}{n} = \frac{\pi}{n}$. The value of the resultant impressed E. M. F. at any moment during this interval is easily found. Denote it by E, then

$$E = e_1 + e_2 + \ldots\ldots e_{\frac{n-1}{2}}$$

$$= K \left\{ \sin\left(\theta + \frac{\pi}{n}\right) + \sin\left(\theta + \frac{\pi}{n} + \frac{2\pi}{n}\right) + \ldots. \right.$$
$$\left. + \sin\left\{\theta + \frac{\pi}{n} + \left[\frac{(n-1)}{2} - 1\right]\frac{2\pi}{n}\right\}\right.$$

$$= K \left\{ \sin\left(\theta + \frac{\pi}{n}\right) + \sin\left(\theta + \frac{\pi}{n} + \frac{2\pi}{n}\right) + \ldots\ldots \right.$$
$$\left. + \sin\left\{\theta + \frac{\pi}{n} + \left[\frac{n-3}{2}\right]\frac{2\pi}{n}\right\}\right.$$

$$= \left[\frac{K_1 \sin\left(\theta + \frac{\pi}{n} + \frac{n-3}{4}\ \frac{2\pi}{n}\right)}{\sin\frac{\pi}{n}}\right] = K_2 \sin\left(\frac{\pi}{2} + \theta - \frac{\pi}{2n}\right)$$

$$= K_2 \cos\left(\theta - \frac{\pi}{2n}\right)$$

It is evident that the resultant impressed E. M. F. E varies during the interval from $\theta = 0$ to $\theta = \frac{\pi}{n}$ just like $\cos(\theta - \frac{\pi}{2n})$; that is to say, it varies just like a simple harmonic. When $\theta = \frac{\pi}{2n}$, E reaches a maximum which is equal to K_2, it has a minimum both when $\theta = 0$ and when $\theta = \frac{\pi}{n}$, each of these minima equals $K_2 \cos \frac{\pi}{2n}$. The ratio of the minimum to the maximum value equals $\cos \frac{\pi}{2n}$. For a three-phase system this ratio is .866, and it diminishes very rapidly as n increases. It is evident that after θ has reached the value $\frac{\pi}{n}$ the armature is, as far as concerns the resultant impressed E. M. F. in exactly the same position as at the start when $\theta = 0$. We conclude therefore that E has $2n$ equal maxima and $2n$ equal minima during each revolution of the armature. In diagram Fig. 5 these fluctuations of E for a three-phase system are represented graphically.

CASE 2.

Similar relations hold good when n is even. The maxima take place when $\theta = 0, \frac{2\pi}{n}, \frac{4\pi}{n}, \ldots\ldots$ The minima when $\theta = \frac{\pi}{n}, \frac{3\pi}{n}, \frac{5\pi}{n}, \ldots\ldots$ and the ratio of any minimum to any maximum is $\cos \frac{\pi}{n}$. Since the magneto-motive force varies according to the same law as the resultant impressed E. M. F., it follows that the strength of the rotary magnetic field fluctuates periodically, having $2n$ equal maxima and $2n$ equal minima during each revolution and the ratio of any minimum to any maximum equals $\cos \frac{\pi}{2n}$. That is, when n is odd, but when n is even then there are only n maxima and n minima and the ratio of any minimum equals $\cos \frac{\pi}{n}$.

A polyphasal generator of this kind would produce a rotary magnetic field of constant strength only when $n = \infty$. For a three phase system the maximum variation would be nearly 14 per cent. of the maximum value. *This agrees perfectly with Mr. Dobrowolsky's calculations, but I fail to see how these calculations could justify any one to assume that they hold good for all types of polyphasal generators* [2]. The generator which we have considered could be actually constructed but its output would be so small in proportion to its size that we may dismiss it at once as an impracticable machine. We can make it practicable by substituting for the non-magnetizable ring which carries the armature coils a laminated iron ring, and for the uniform magnetic field, the magnetic field of a well made field magnet with its pole pieces placed with respect to the armature coils

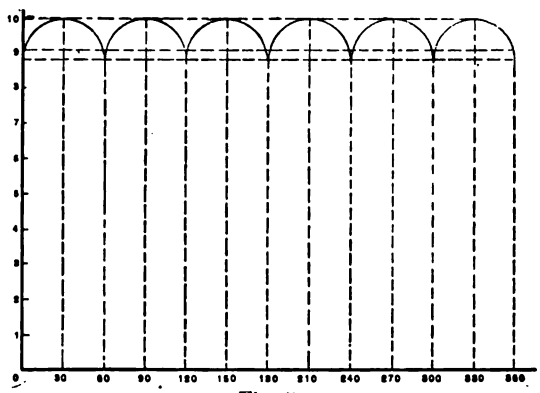


Fig. 5.

in any one of the various ways sanctioned by practical experience. But in a generator of this kind the resultant impressed E. M. F. will no longer vary according to the law which I have pointed out a little while ago. To be sure, we shall still have the same number of maxima and minima, as may be inferred readily from our knowledge of the shape of the E.M.F. curve of a continuous current dynamo. We all know that this curve is not in general a straight line, but a wave line having as many maxima and as many minima as there are sections on the commutator. But the ratio of the maxima to the minima is no longer an a priori calculable quantity. If we knew the mathematical relation between the intensity of the field

(2) M. v. Dolivo-Dobrowolsky : Der Drehstrom und seine Entwickelung; Officielle Austellungs Zeitung, Electricitaet, Heft 12.

at any point of the armature surface and the co-ordinates of this point with respect to the neutral plane then we could calculate that ratio, but the amount of experimental and practical work involved in this problem would be very great. *A much easier and practically much more important problem is to determine the conditions which must be fulfilled in the construction of a polyphasal generator, in order that it may be capable of producing a rotary magnetic field of practically constant intensity in the simplest possible way, that is without the application of brushes and commutators, and also without employing too many phases.* Mr. v. Dolivo-Dobrowolsky seems to think that a three phasal generator is incapable of doing that, for he distinctly says that such a generator necessarily produces a rotary magnetic field whose strength varies 14 per cent. He also states that (evidently to obviate these fluctuations) the Allgemeine Electricitæts Gesellschaft employ a method of transmitting currents of smaller dif-

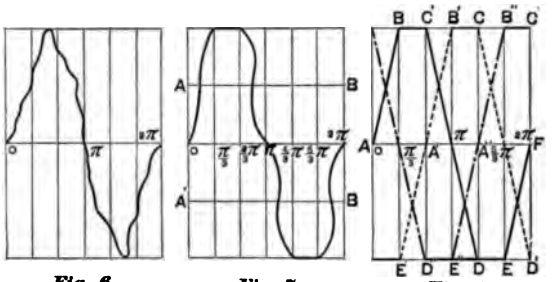


Fig. 6. *Fig. 7.* *Fig. 8.*

ferences of phase than one-third of the period through three wires. In this point they claim to be ahead of Tesla, Bradley, Haselwander and Wenstrom. In fact if one is not exceedingly careful in the perusal of Dobrowolsky's discussions of this subject he will be led to believe that the rotary field in some of Tesla's motors varied as much as 40 per cent. and certainly not less than 14 per cent. I do not think that Mr. Dobrowolsky wishes to be understood as holding that opinion; for neither he nor anybody else excepting Tesla himself can know what these variations were. The number of phases employed tells us nothing definite about the range of these variations.

A polyphasal dynamo which is capable of producing a rotary magnetic field of constant intensity must be constructed in such a way that its resultant magneto-motive-force must remain constant as long as speed and the magnetic field of the field magnets re-

main constant. As long as the variable electromotive force developed in each coil follows the law of a simple harmonic that result can never be accomplished by a finite number of phases, but it may, perhaps, be accomplished by producing in each coil a variable electromotive force which varies according to some definite complex harmonic law. In a well made commercial machine the electromotive forces developed in the various turns of the armature always vary according to some such a law. The form of this complex harmonic law depends on the form of the magnetic field of the field magnets and also on the distribution of the coils over the armature. The problem that remains to be investigated consists therefore of three parts: 1st. What must be the particular form of the complex harmonic E. M. F. developed in each coil of a polyphasal generator, in order that both the condition of continuity be fulfilled and also that the resultant impressed E .M. F. be continually constant. 2nd. What form of the

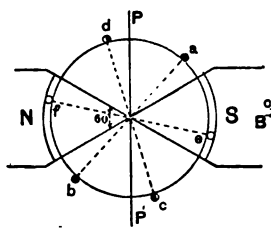


Fig. 9

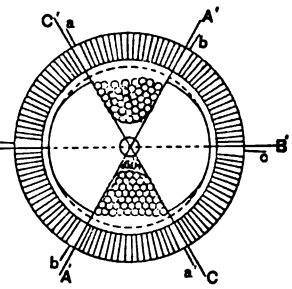


Fig. 10.

magnetic field of the field magnets will be capable of producing such an E. M. F. 3d. Can a continually constant resultant E. M. F. produce a rotary field of constant strength.

1st. The first part of this problem is purely mathematical. In a paper read before the New York Mathematical Society I indicated a method of discussing this part in a general way, and worked out completely two particular cases, namely the cases of a three and four phasal system. The paper is given in the appendix.

2d. For a three phasal system the form of the complex harmonic E. M. F. given in Fig. 7. will satisfy all the conditions. The form A, B, C, E, F, given in Fig. 8, is only a particular case and ought to be aimed at in the construction of the machine.

When there are only three turns within a space through which the armature moves with respect to the field during the time that corresponds to a complete period as in the case of the Lauffen

generator (see Figs. 11 and 12), then the field of the field-magnets must be constant in intensity during an angle which corresponds to one-sixth of the period. I have indicated that, in the diagram Fig. 9. In the case of bipolar three phasal generators as indicated in the diagram Fig. 10, where we have six coils, the diametrically opposite pairs being connected in series; the pole faces must

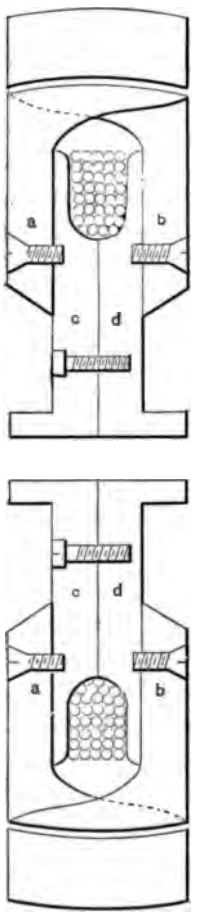


Fig. 11.

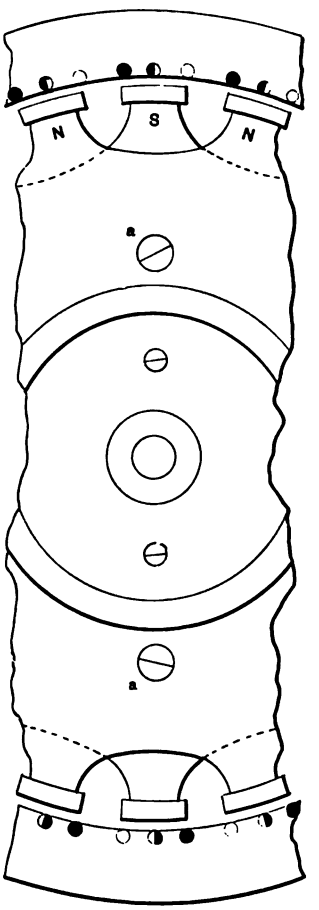


Fig. 12.

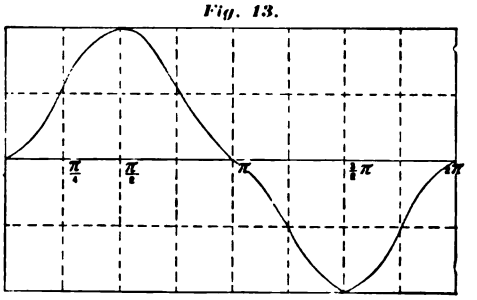
have an angular width of 120 degrees and the field must be constant in intensity within the region bounded at any moment by the armature and the pole faces. This is a practical problem offering no serious difficulties judging from the experimental results obtained by S. Thompson, Isenbeck, Mordey and others,

and also from the experimental results obtained lately by a graduate of our school, Mr. Freedman, John Tyndall Fellow of Columbia College.

The curve of impressed E. M. F, which must be produced in the case of a four phasal generator is given in Fig. 13, and needs no further commentary. Larger number of phases offer no special advantages whereas the disadvantages arising from employing a large number of phases are self-evident.

3d. When a coil, in which a simple harmonic E.M.F.is developed is closed by a resistance, whether self-inductive or non-self-inductive, the current which is set up in the closed circuit will be a simple harmonic, having therefore all the characteristics of the impressed E. M. F. This, however, is not necessarily the case when the impressed E. M. F. is a complex harmonic. A complex harmonic E. M. F. is composed of a large number of simple harmonic E. M. forces of different frequencies, all the higher fre-

Fig. 13.



quencies being multiples of the fundamental frequency. When, therefore, a coil in which a complex E. M. F. is generated, is closed by a conductor, and the current is started,the current will be also a complex harmonic, each simple harmonic component of the complex harmonic E. M. F. producing its own simple harmonic current which is a component of the resultant complex harmonic current. But since the component simple harmonic E. M. forces have each a different frequency, it follows that they will have a different impedance and the shifting of phase will be also different for each component current, currents of higher frequency having a larger shifting in phase and also the ratio of the amplitude of any one of the component currents to the amplitude of any other component of lower frequency, being smaller than the ratio of the amplitudes of the corresponding component E. M. forces. In this respect the propagation of the complex harmonic

current-wave resembles very much the propagation of a complex harmonic sound-wave or a complex harmonic light-wave through an absorptive medium like air. The component simple harmonic waves of light and sound will in general suffer the less through the transmission the longer their wave-length. Just as the sound and light-waves, after such a transmission, lose a great many characteristics of the original vibration which produced them, so an electric wave in its transmission through a conductor possessing ohmic resistance and electro-magnetic, not to speak of the electro-static, inductance will lose a great many characteristics of the impressed E. M. F.

To put this into simple symbolic language of mathematics,

Let L be the coefficient of self-induction of the circuit,

" R be the total resistance,

" $K \sum_1^\infty {}_m\, a_m \sin m\, p\, t$ be the complex harmonic impressed E. M. F. where $p = 2\pi \times$ fundamental frequency,

" x be the value of the current at any moment t.

We shall have, then,

$$L \frac{d\,x}{d\,t} + R\,x = K \sum_1^\infty {}_m\, a_m \sin m\, p\, t.$$

The solution of this differential equation gives

$$x = K \sum_1^\infty {}_m \frac{a_m}{\sqrt{R^2 + m^2\, p^2\, L^2}} \sin\,(m\, p\, t - \varphi_m)$$

$$\text{where } \tan \varphi_m = \frac{m\, p\, L}{R}$$

The current x is a complex harmonic, its component simple harmonic eurrents being

$$x = x_1 + x_2 + \ldots\ldots + x_m + \ldots\ldots \textit{ ad infin.}$$

$$\text{The current } x_\alpha = \frac{K\, a_\alpha}{\sqrt{R^2 + a^2 p^2 L^2}} \sin\,(a\, p\, t - \varphi_\alpha)$$

$$\tan \varphi_\alpha = \frac{a\, p\, L}{R}$$

Let E be the impressed E. M. F., then

$$E = e_1 + e_2 + e_3 + \ldots\ldots + e_m + \ldots\ldots \textit{ ad infin.}$$

The component simple harmonic E. M. F. e_α is given by

$$e_\alpha = a_\alpha \sin a p t$$

These relations give an exact quantitative expression to the preceding physical description.

These considerations made me hesitate at first in taking as granted that a polyphasal generator producing complex E. M. forces, such as I deduced mathematically in the course of my paper, would be capable of producing a rotary magnetic field of constant intensity. But I was glad to find out that my hesitation was groundless, at any rate in certain particular but important cases.

Consider the three-phasal generator whose diagram is given in Fig. 10. Take, now, another *well-laminated* armature wound in a similar way as the armature of the generator. Connect the three pairs of coils of the generator to the three sets of coils in armature 2. We shall have three separate circuits, the ohmic resistance and the self and mutual inductance in each circuit being the same. Denote by E_1 E_2 E_3 the three complex harmonic E. M. forces in the three circuits. Let x, y, z be the currents at any moment. Then we shall have

$$L \frac{dx}{dt} + M \frac{dy}{dt} + M \frac{dz}{dt} + R x = E_1$$

$$L \frac{dy}{dt} + M \frac{dx}{dt} + M \frac{dz}{dt} + R y = E_2$$

$$L \frac{dz}{dt} + M \frac{dx}{dt} + M \frac{dy}{dt} + R z = E_3$$

But since $E_1 + E_2 + E_3 = 0$ for all values of t it follows that

$$L \frac{d}{dt}(x + y + z) + 2 M \frac{d}{dt}(x + y + z) + R(x + y + z)$$

$= 0$ for all values of t. This can be true only if

$$x + y + z = 0$$

for all values of t. That is to say, the currents fulfill the condition of continuity. We can therefore employ the method of polyphasal connection. Substitute now in the first of the three differential equations

$$z = -(x + y)$$

and we obtain

$$(L-M)\frac{dx}{dt}+Rx=E_1=K\sum_1^{\infty}{}_m\, a_m \sin m\,p\,t$$

The solution of this equation gives

$$x=K\sum_1^{\infty}{}_m \frac{a_m}{\sqrt{R^2+m^2p^2(L-M)^2}}\sin(m\,p\,t-\varphi_m)$$

Similarly

$$y=K\sum_1^{\infty}{}_m \frac{a_m}{\sqrt{R^2+m^2p^2(L-M)^2}}\sin\left\{m\left(pt+\frac{2\pi}{3}\right)-\varphi_m)\right\}$$

$$z=K\sum_1^{\infty}{}_m \frac{a_m}{\sqrt{R^2+m^2p^2(L-M)^2}}\sin\left\{m\left(pt+\frac{4\pi}{3}\right)-\varphi_m\right\}$$

In the case under consideration both L and M are pretty small when the metallic parts of the magnetic circuits are near the saturation point, so that $L-M$ is small, and $m^2p^2(L-M)^2$ may be small in comparison to R^2 even for large values of m, unless the frequency is very high. Also, since

$$\tan\varphi_m=\frac{m\,p\,(L-M)}{R}$$

φ_m is exceedingly small unless p is very large, we shall have for moderate frequency generators

$$x=\frac{K}{R}\sum_1^{\infty} a_m \sin m\,p\,t$$

and similarly for y and z. The same method of reasoning may be easily applied to any number of phases. The mathematical operations will be considerably larger, but still the same results will be deduced without much difficulty.

That is to say, the curves for the currents are the same complex harmonics as those of the impressed E. M. F. The currents therefore, produce a rotary magnetic field of constant intensity; this is evidently true even if these currents produce a saturation in the iron part of the magnetic circuits

The resemblance between a polyphasal generator and a continuous current dynamo, which these relations bring into view, is exceedingly striking and instructive.

The advantages gained from a polyphasal generator capable of producing a rotary magnetic field of constant intensity would be very much diminished indeed if it should turn out that it is impossible to devise a simple and efficient method of transformation [3] by means of which the polyphasal system of currents producing a rotary field of constant intensity (a constant rotary field system) can be transformed any number of times without losing its distinguishing characteristic. I intended to discuss this problem also, this evening but having been disappointed by the mechanician who is constructing several pieces of apparatus illustrating this problem I decided to postpone this discussion to some other time.

To sum up:

1st. The consideration of simple harmonic impressed E. M. F's does not tell the whole story of the polyphasal generators.

2nd. The law of variation of the strength of rotary magnetic field which a polyphasal generator can produce is not as simple as Mr. v. D. Dobrowolsky thinks.

3rd. Polyphasal coupling transformers must be worked at low magnetizations and low frequencies, otherwise they will not satisfy the condition of continuity. It follows, therefore, that they will probably be very large for the output which they can give.

4th. It is very probable that nearly constant rotary magnetic fields can be produced in practice by a small number of phases; perhaps not more than three.

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(3) Not only transformation of the power supplied by the generator into electrical power of higher or lower potential, but also transformation of this power into mechanical power. This, of course, leads into the discussion of rotary magnetic fields produced under practical conditions.

ON A PECULIAR FAMILY OF COMPLEX HARMONICS.

Read before the New York Mathematical Society, December 5, 1891.

The following investigation was suggested by the practical problem: Can a polyphasal generator be constructed which will be capable of producing a rotary magnetic field of constant intensity? It seems, therefore, sufficient for the present to discuss only those features of the mathematical side of this question which have a direct bearing upon its practical side.

The mathematical theory of polyphasal generators involves a discussion of harmonic functions, or *harmonics*, to use a shorter expression.

A *simple harmonic* is defined by the following expression:—

$$a + b \sin (x + \alpha).$$

This harmonic differs in phase only from the harmonic

$$a + b \sin x.$$

The angle α is called their *difference of phase.*

A *complex harmonic* is defined by the following expression:

$$\sum_{o}^{n} a_\alpha \sin \alpha x + \sum_{o}^{m} C_\alpha \cos \alpha x$$

If n and m are infinite the complex harmonic is sometimes called infinitely complex. The multiples α are integral numbers.

Consider a periodic function $f(x)$ whose period is $2\pi = \varphi$. If this function with its differential coefficients is finite, continuous and singly valued for all values of x, with the exception of certain values which are at finite distances from each other, and if in addition

$$f(-x) = -f(x) \text{ and } f(o) = o,$$

then by Fourier's theorem

$$f(x) = a_1 \sin x + a_2 \sin 2x + a_3 \sin 3x + \ldots\ldots + a_m \sin m x + ad\ infin.$$

$$\text{where } a_m = \frac{2}{\pi} \int_{o}^{\pi} f(x) \sin m x \, dx$$

It is required now that $f(x)$ fulfill the following two conditions:

1st, $f(x) + f(x + \frac{\varphi}{n}) + f(x + 2\frac{\varphi}{n}) + \ldots\ldots$

$$+ f[x + (n-1)\frac{\varphi}{n}] = 0 \text{ for all values of } x$$

2d, $f(x) + f(x + \frac{\varphi}{n}) + f(x + 2\frac{\varphi}{n}) + \ldots\ldots\ldots$

$$+ f(x + \frac{n-1}{2}\frac{\varphi}{n}) = \text{const. for all values of } x \text{ betw. } 0$$

and $\frac{\varphi}{2n}$ when n is odd, and

$$f(x) + f(x + \frac{\varphi}{n}) + \ldots\ldots + f(x + \frac{n-2}{2}\frac{\varphi}{u}) = \text{const.}$$

for all values of x betw. 0 and $\frac{\varphi}{n}$ when n is even.

The coefficients $a_1\, a_2 \ldots\ldots a_m \ldots\ldots$ can of course be always so determined as to fulfil not only these two, but also any other number of possible conditions.

The first condition, which I call the *condition of continuity*, can be written

$$a_1 \sin x + a_2 \sin 2x + \ldots\ldots + a_m \sin mx + \ldots\ldots$$

$$+ \quad a_1 \sin (x + \frac{\varphi}{n}) + a_2 \sin 2(x + \frac{\varphi}{n}) + \ldots\ldots\ldots$$

$$+ a_m \sin m(x + \frac{\varphi}{n}) + \ldots\ldots$$

$$+ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

$$+ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

$$+ \quad a_1 \sin \left\{x + (n-1)\frac{\varphi}{n}\right\} + \ldots\ldots$$

$$+ a_m \sin m \left\{x + (n-1)\frac{\varphi}{n}\right\} + \ldots\ldots = 0$$

$$\text{or } \sum_1^\infty{}_m\, a_m \frac{\sin m\,(x + \frac{n-1}{2}\frac{\varphi}{n}) \sin \frac{m\varphi}{2}}{\sin \frac{m}{2}\frac{\varphi}{n}} = 0 \ldots\ldots (1)$$

This function I call the *resultant of n identical component harmonics* $f(x)$, differing from each other in phase, only, by one nth of the period. It is evident that every term in the resultant vanishes, excepting those terms whose multiple of x is divisible by n. The resultant can therefore be written

$$\sum_{1}^{\infty}{}_{m} (-1)^{a(n-1)} C_{\alpha} \sin a n x = F(x)$$

Since

$$C = \frac{4}{\varphi} \int_{0}^{\frac{\varphi}{2}} F(x) \sin a n x \, d x$$

it follows that if $F(x) = 0$ for all values of x betw. $x = 0$ and $x = \frac{\varphi}{2}$ then every coefficient C_{α} is zero. *That is to say,* $f(x)$ *will fulfil the condition of continuity if it contains no terms of the form* $a_m \sin m x$ *where* m *is divisible by* n.

Those acquainted with the theory of dynamo-electric machinery will easily translate this into the following physical language: *The algebraical sum of all the electromotive forces generated at any moment in the various coils of a polyphasal dynamo will not necessarily fulfil the condition of continuity; that is, will not necessarily vanish. The form of the magnetic field must be such that the harmonic electromotive force generated in each coil does not contain in its mathematical expression a multiple of the variable angle which is divisible by the number of the phases.*

The first part of the second condition can be written

$$a_1 \sin x + a_2 \sin 2 x + \ldots\ldots + a_m \sin m x + \ldots\ldots$$

$$+ a_1 \sin (x + \frac{\varphi}{n}) + \ldots\ldots + a_m \sin m (x + \frac{\varphi}{n}) + \ldots\ldots$$

$$+ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

$$+ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

$$+ a_1 \sin (x + \frac{n-1}{2} \frac{\varphi}{n}) + \ldots\ldots + a_m \sin m (+ x \frac{n-1}{2} \frac{\varphi}{n})$$

$$+ \ldots\ldots = \text{const.}$$

$$\text{or} \sum_{1}^{\infty}{}_{m} a_m \frac{\sin m\left(x + \frac{n-1}{4}\frac{\varphi}{n}\right) \sin \frac{m(n+1)}{4}\frac{\varphi}{n}}{\sin \frac{m}{2}\frac{\varphi}{n}} = c.$$

for all values of x between $x = 0$ and $x = \frac{1}{2}\frac{\varphi}{n}$

The second part of this condition may be written

$$\sum_{1}^{\infty}{}_{m} a_m \frac{\sin m\left(x + \frac{n-2}{4}\frac{\varphi}{n}\right) \sin m\frac{\varphi}{4}}{\sin \frac{m}{2}\frac{\varphi}{n}} = c \quad \ldots\ldots (2)$$

for all values of x betw. $x = 0$ and $x = \frac{\varphi}{n}$

From these two relations, I infer the following physical theorem: *If the terminals of the armature coils of a polyphasal dynamo capable of producing a constant resultant rotary impressed* E. M. F. *be connected to a commutator and the machine be run as a direct current dynamo, it will give an absolutely constant electromotive force, speed and field intensity being maintained constant.*

It must be observed, however, that in the case *when the number of phases is an odd number, then the armature should have twice as many coils as there are phases, and when the machine is run as a polyphasal dynamo, then the diametrically opposed pairs are connected in series.*

Relations (1) and (2) enable us to determine in each case the simplest forms of the component harmonics which will satisfy these two relations. As the simplest forms I define those forms which can be produced in polyphasal generators by the simplest devices of construction.

Particular Cases.

Any further general discussion would have only an indirect bearing upon the practical side of the problem which I propose to discuss at present. I therefore pass to particular cases.

Let $n = 2$.

The above relation gives

$$\sum_{1}^{\infty}{}_{m}\, a_m \sin m\, x = c$$

for all values of x between $x = o$ and $x = \pi$.
except when $x = o$ and $x = \pi$.

This solution has no practical value. I therefore dismiss it.

Let $n = 3$.

The relation given above reduces to

$$\frac{\sum_{1}^{\infty}{}_{m}\, a_m \sin m\,(x + \frac{\pi}{3}) \sin m\, \frac{2\pi}{3}}{\sin \frac{m\pi}{3}}$$

or $$\sum_{1}^{\infty}{}_{m}\, a_m\, 2 \sin m\,(x + \frac{\pi}{3}) \cos \frac{m\pi}{3} = c$$

for all values of x betw. $x = o$ and $x = \frac{\pi}{3}$.

Since $f(x)$ has only two conditions to fulfil, all the coefficients a_m, where m is an even number, may be suppressed and still leave more than a sufficient number of coefficients, their number being infinity. In that case the last relation reduces to

$$\sum_{1}^{\infty}{}_{m}\, a_m \sin m\,(x + \frac{\pi}{3}) = c.$$

$$\text{or} \sum_{1}^{\infty}{}_{m}\, a_m \sin m\, \xi = c$$

for all values of ξ betw. $\xi = \frac{\pi}{3}$ and $\xi = \frac{2\pi}{3}$.

That is to say, the function $f(x)$ itself must be such as to have a constant value between $x = \frac{\pi}{3}$ and $x = \frac{2\pi}{3}$ and, therefore, also between $x = \frac{4}{3}\pi$ and $x = \frac{5}{3}\pi$.

Since $f(x) + f(x + \frac{2\pi}{3}) =$ const. for all values of x between $x = o$ and $x = \frac{\pi}{3}$, I infer that the curve $f(x)$ may have

any one of those forms, one of which is given in Fig. 7 (of the preceding paper). That is, the curve is symmetrical both with respect to the axis of x and also to the bisector $A\ B$ and $A'\ B'$ of the upper and lower maxima.

The curve given in Fig. 8 (of the preceding paper) would perhaps be aimed at in the construction of polyphasal dynamos. Its equation is

$$f(x) = K\ (\sin x - \frac{1}{5^2} \sin 5\,x + \frac{1}{7^2} \sin 7\,x - \frac{1}{11^2} \sin 11\,x + \ldots\ldots)$$

Let $n = 4$

Equation (2) gives

$$\sum 2\,a_m \sin m\,(x + \frac{\pi}{4}) \cos m\,\pi = c$$

and therefore

$$\sum \frac{2\,a_m}{m} \cos m\,(x + \frac{\pi}{4}) \cos m\,\frac{\pi}{4} = -\,c\,(x - \frac{\pi}{4})$$

$$= -\,\frac{c}{2}\,x - \frac{c}{2}\,x + \frac{c\,\pi}{4}$$

for all values of x between $x = o$ and $x = \frac{\pi}{2}$

The last equation can also be written

$$\sum \frac{a_m}{m} \cos m\,x - \sum \frac{a_m}{m} \sin m\,\frac{\pi}{2} \sin m\,x$$

$$= -\,\frac{c}{2}\,x - \frac{c}{2}\,(x - \frac{\pi}{2})$$

This relation enables us to determine the form of the component complex harmonic function $f(x)$ which will fulfil all the conditions. It is easily seen that there is in this case just as in the case of three phases an infinitely numerous family of complex harmonics which will fulfil these conditions. The simplest harmonic is obtained by putting

$$\sum \frac{a_m}{m} \cos m\,x = -\,\frac{c}{2}(x - \frac{\pi}{2}.)$$

$$\therefore \frac{a_m}{m} \cdot \frac{\pi}{4} = -\frac{c}{2}\int_0^{\frac{\pi}{2}} x \cos m x \, d x + \frac{c\,\pi}{4}\int_0^{\frac{\pi}{2}} \cos m x \, d x$$

$$= \frac{c}{2\,m^2}$$

and therefore

$$a_m = \frac{2\,c}{m\,\pi}$$

I conclude therefore that the harmonic

$$f(x) = \frac{\sin x}{1^2} - \frac{\sin 3\,x}{3^2} + \frac{\sin 5\,x}{5^2} - \frac{\sin 7\,x}{7^2} + \ldots\ldots ad\ infin.$$

fulfils all the conditions. Its form is given in Fig. 14. (It is only a special case of the curve given in Fig. 13 of the preceding paper.)

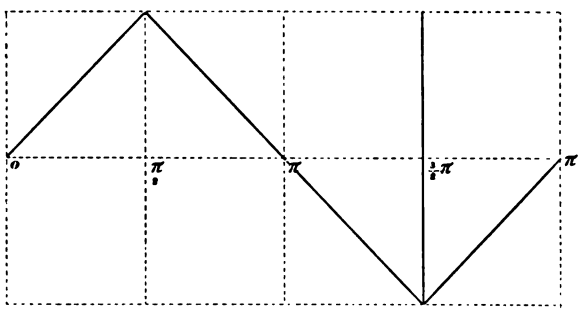

Fig. 14.

The component harmonics fulfilling these conditions may be deduced similarly for any other value of n. But as the problem which I originally proposed to myself does not extend beyond these limits which I have just reached I prefer to postpone further considerations of the subject.

The character of these curves points out clearly the physical fact that it is an easy matter to construct three and four phase generators which will be capable of producing a constant resultant impressed E. M. F. Whether such generators will also be capable of producing a rotary magnetic field of practically constant intensity is a problem which I propose to discuss in a series of papers which will be presented shortly before the American Institute of Electrical Engineers.

Discussion.

The Chairman [Prof. Thomson]:—You have heard the interesting and instructive paper by Dr. Pupin. I would say that it is perhaps the first exposition of some of the principles underlying this system of polyphasal transmission that I have seen, and I hope that the discussion may be indulged in by the members so that we may have some points cleared up by those conversant with the subject. It is now open for discussion.

Mr. Chas. P. Steinmetz:—Having had the pleasure of hearing Prof. Pupin's paper at the New York Mathematical Society some days ago, I might be allowed to make the same remark I made there—that it would be perhaps more advisable to use a shape of the electromotive force similar to Fig. 7, than the shape in Fig. 8, because, as you see, in shape Fig. 8 the curve has a sharp corner, and even if we could produce waves of electromotive force that have sharp corners, it can hardly be expected that

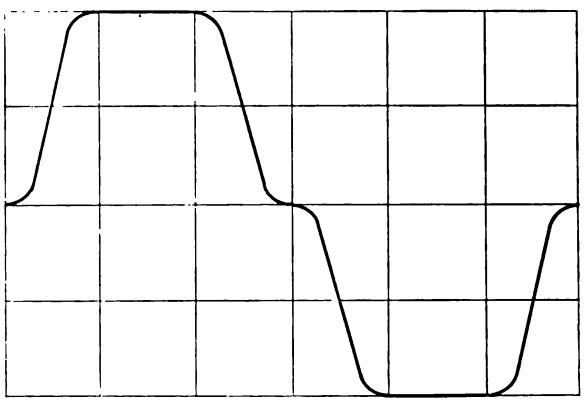

Fig. 15.

we can derive from such a sharp-cornered e. m. f. a current which, after being sent through step-down transformers, over lines of considerable electrostatic capacity, and again through step-down transformers into a motor of high self-induction, would still have retained this sharp-cornered shape. But the theory requires that the electric *current* in the motor has the shape given in Figs. 6 to 8. Even if the e. m. f. had the shape of Fig. 8, the current in such a highly inductive circuit would differ considerably, having lost the sharp corners, etc. Hence Fig. 8 would be less commendable. But the shape of Fig. 7 might easily be chosen, so that there would be no sharp corners, but a steady and continuous variation, as shown in Fig. 15.

Then, with regard to the equations of the currents x, y, z, on page 576, I wish to make a remark. As stated by Dr. Pupin, the complex harmonic of the electromotive force produces a current

which is a complex harmonic too, and it is identically the same complex harmonic as long as the circuit has no self-induction and no capacity or, what amounts to the same, as long as self-induction and capacity have a certain ratio with each other. But as soon as the circuit has self-induction, the complex harmonic of current differs from the complex harmonic of electromotive force, and differs the more, the heavier the self-induction of the circuit is, by the decrease of the higher terms of this infinite series of simple harmonics which constitute the complex harmonic of electromotive force, these having a high self-induction; and therefore if we produce such a type of electromotive force and let it send a current through an inductive circuit, it will probably break to pieces entirely and show a current resembling a simple sine-wave as closely as one egg to another. In the equations for x, y, z, on page 576, as the condition that the shape of the current does not differ from the shape of the electromotive force, was found that the second term, $m^2 p^2 (L - M)^2$, can be neglected against the first term, R^2. This means, in *plain language*, that the shape of the current-wave is the same as that of the E. M. F., if the *self-induction of the circuit is negligible.* For the first term, R, is the *resistance;* the second term, $m\,p\,(L - M)$, is the *inductance* of the circuit.

Hence neglecting the second term means neglecting the inductance—that is, it means that the motor circuit has no self-induction. Now, anybody who ever tried to design an alternating motor, has found out to his disgust, generally, that the self-induction of such a motor, even under the most favorable conditions, is anything but negligible. Unfortunately, I could not get any data on these rotary motors, but on some other alternate current motors I can give data. In the Ganz and Company synchronous motor, the plant efficiency is claimed—by the manufacturers—to be 90 per cent. This *unusually* high plant efficiency, this *unusually* low retardation, might be explained by the fact that the motor is synchronous and the field fed by rectified alternate currents, that only the self-induction of the armature is in circuit, and the field adds no self-induction whatever. Furthermore, that the frequency used in those motors, 42 periods per second, is somewhat less than the one-third of the frequency of our American alternators. If this current were a complex harmonic we would have in the main wave 26 degrees retardation and 90 per cent. plant efficiency —that is, the intensity of the current would be 90 per cent. of that value it would have with no self-induction present. The second harmonic has only the plant efficiency of 72 per cent.; the third harmonic of 7, the fourth of 47, the tenth only 20 per cent.—that is, is decreased to 20 per cent., while the first wave is decreased only to 90 per cent. Hence, even in a motor circuit of such unusually low self-induction, if a wave of shape Fig 8 is applied, it will come out entirely broken up, so I do not think that really the self-induction can be neglected. The more, as just

the first term, R, is small, because we do not want to have the resistance of the circuit large, for the resistance determines the loss of power, and we do not want to have so much loss of power. We want to run motors with these currents. Indeed, if we run a motor from this alternating current, we get a counter-electromotive force in the motor, and to make these same equations hold we might represent this counter-electromotive force by apparent resistance. But in such a motor, the counter-electromotive force is not of equal phase with the current, but lags behind the current the more, the lighter the load, and will come nearer in phase to the phase of the current when we increase the load. Hence, the apparent inductance is not even a constant, but a variable of the circuit, and exceedingly variable, too. The inductance is small, almost *nil*, if the motor is at rest under full head of pressure. As soon as the motor starts, its self-induction increases, up to a value which corresponds to the load the motor is carrying, so that if the motor is heavily loaded the inductance is *comparatively* small, though very far from negligible, while, when the motor is running light, its self-induction increases to such a value as to almost entirely shut off the current.

Now I come to the consideration of this quantity $(L - M)$. This quantity is really nothing but, or rather proportional to, that amount of magnetism, or that magnetism which constitutes the rotating magnet poles. So if the motor is at rest, heavy eddies in the short circuit armature-circulating coils, the useful magnetism is almost *nil;* almost no magnetism passes through the armature. Hence L is almost identical with M. The circuit has almost no self-induction. If the motor starts, runs with heavy load, then a certain amount of magnetism passes through the armature. L is different from M. L has increased, and we get a difference of phase of the current and a different shape of the current wave. Now, suppose the motor runs with almost no load, then the self-induction of the motor is very large and we can neglect R, the *first* term, *entirely.*

In this case the original shape of the electromotive force curve is entirely broken up and has changed into a somewhat harmonic shape. So I cannot think there is any hope to transfer any other shape of the curve but a simple harmonic through a circuit of heavy and very variable self-induction. That would be rather disappointing and very disagreeable for the builders of motors if the working of such rotary motors depended upon a certain shape of alternating current waves. Indeed, we all have heard and read half a dozen times—some of us even oftener—how bad and worthless the Tesla motor is, because there the fluctuations of the magnetism amount to, I believe, 41 per cent. and how grand and beautiful the improvements of Mr. Dolivo von Dobrowolsky are, because in his motor the fluctuations amount to only 14 per cent., and if having heard and read something very often proves its truth, then it must certainly be true. At least this statement

about the fluctuations of the intensity of the rotary magnetism seems generally to have been accepted as true. For I never heard any doubt expressed on the correctness of this fluctuation theory. And, nevertheless, in the very first publication of Ferraris on rotating magnet poles, in the very publication which introduced this rotary magnetism to the public years ago, it has been shown that if you sent two alternating currents, one lagging behind the other by one-quarter of a period, through two coils at right angles with each other, those two currents produced in the centre of those coils a magnetic field which revolves with *constant strength* and *constant speed*.

Let A in Fig. 16 represent the one, B the other one, of the two perpendicular coils, which are excited by two alternating currents

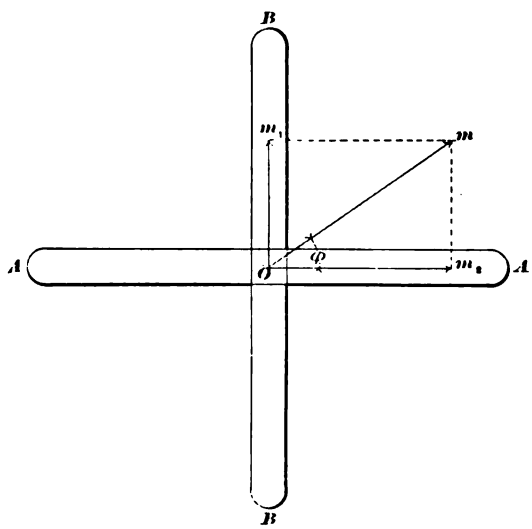

FIG. 16.

of 90 degrees difference of phase. Then the magnetism produced by coil A at any time can be represented by the line,

$$\overline{O\,m_2} = M \sin \varphi$$

The magnetism produced by coil B at the time t is

$$\overline{O\,m_2} = M \cos \varphi$$

where

$$\varphi = \frac{2\pi}{T}\, t$$

T being the time of one complete period.

These two magnetisms, $\overline{O\,m_1}$ and $\overline{O\,m_2}$, combine by the law

of *parallelogram*, which as a consequence of the law of *conservation of energy* holds, to the resulting magnetism:

$$O\bar{m} = M$$

of constant strength, at the phase:

$$m_2\, O\, m = \varphi = \frac{2\pi}{T}\, t$$

of constant velocity.

That means, the *magnet poles revolve with constant strength and constant velocity, produced by true harmonic or sine-waves.*[1]

Coming, now, to the conclusion, we see

1. It is possible to produce rotary magnetism of constant strength and constant velocity of rotation by means of true sine-waves.

2. It is hopeless to try to produce rotary magnetism of constant strength and velocity by means of a particular shape of the wave of E. M. F., because in a circuit of considerable and variable self-induction the shape of the current wave will differ in a considerable and a variable degree from the shape of the E. M. F. wave for any shape of the E. M. F. but the true harmonic or sine-wave.

3. Hence it is more advisable not to build the *generators* so that they produce that shape of E. M. F. which in a particular type of rotary motors will give magnetism of constant strength and velocity; but to build motors which will give magnetism of constant strength and velocity from true harmonic or sine-waves, as the only waves which can be transformed, transmitted through inductive and other circuits without changing their shape; to build the rotary motors for sine-waves, as the possibility has been shown by Ferraris, and as already in the oldest Tesla motors it evidently has been the aim of the designer. [Applause.]

DR. PUPIN:—Mr. Steinmetz went a little beyond the limits of this paper by talking about the motors. I said at the start that I was going to confine myself to the polyphasal generators and particularly to polyphasal generators which could under certain well defined conditions produce a rotary field of constant strength. I am going to consider in future the question of transformation and the question of rotating field used for driving a motor. Now, in the case that I considered, I simply had an iron ring surrounded by a set of coils and nothing else. I had no motor armature here. In this case, $L - M$ could very easily be made small in comparison to R, the resistance, and therefore may be neglected. But you need not neglect it. If $(L - M)$ is not negligible, then the magneto-motive force will vary, but its variation will be less than 14 per cent. How much less remains to be

1. Kapp: Alternate Current Machinery, page 82.

seen. I don't think that I can show it very well without entering fully into the discussion of the polyphasal motor and transformer. But this subject requires a carefully prepared paper to form a basis on which it can be advantageously discussed. If Mr. Steinmetz will have a little patience, I will promise to give him a chance to discuss these things also.

As far as the Ferraris contrivance is concerned, I never saw that paper to which Mr. Steinmetz refers. There must be a hitch in it, I think. Perhaps Mr. Steinmetz will exactly explain the contrivance, so that we can see the magnetic circuit and see whether his ideas are correct or not. I know a great many mistakes have been made on this very point of the magnetic circuit. Not enough attention was paid as to whether the magneto-motive forces worked in series or multiple arc, nor to the shape and distribution of the magnetic circuits. But still, I would like to know the exact form of the Ferraris motor and the magnetic circuits before I decide to comment upon it.

MR. STEINMETZ:—Ferraris built only a little toy, and his magnetic circuits, so far as I know, were completed in air, not in iron, though that hardly makes any difference. The only possible error there could be is the use of the law of parallelogram in combining M. M. F.'s acting in different directions upon a point, and this law of parallelogram, or polygon, is a consequence of the law of conservation of energy, and therefore its correctness can hardly be questioned. But as soon as you accept that, then the reasoning I have given here must be correct.[1] So there is no possibility of any error if the whole phenomenon takes place in air. Suppose, now, the phenomenon does not take place in air, but in any other medium of constant magnetic conductivity, then you have exactly the same conditions. The air space between armature iron and field iron might introduce some discrepancy, though I hardly think so. But, then, the next problem would simply be how to shape the motor iron, how to distribute the wire coils, to get in the iron circuit separated by the air gap the same magnetic distribution as would take place without any iron in the air.

DR. PUPIN:—As you have heterogeneous media, you cannot have complete homogeneous magnetic circuits, and that is where the difficulty comes in. Your parallelogram of magnetizing forces will not apply here, and you are forced to be satisfied with the application of Ohm's law to magnetic circuits, which will not give you the result you claimed a little while ago.

MR. STEINMETZ:—Consider that wire coils of iron are closely embedded in iron, then there is no question that the same phenomenon takes place in the iron as in Ferraris's experiment in air. So the only problem would be how to shape the iron practically,

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1. Exactly the same conclusion I find now given in Kapp, Alternate Current Machinery, page 82.

to get rotary magnetism of constant strength and velocity from sine-waves.

This three-phase current system has been brought up the last time as something entirely new. I cannot agree with that in the least. For already in the old Tesla motor the three phases of current, only that in the three wires that go out from the central station the three currents have not a difference of phase of exactly 120 degrees, but two have a difference of 90 degrees, and either one of these two currents has with the third a difference of phase of 135 degrees. But if now the "Allgemeine Electricitæts Gesellschaft" transmits currents of less than 120 degrees difference of phase—well, then the Dobrowolsky system comes back exactly to the old three-wire system of Tesla, only that the motor is certainly built somewhat differently. But that does not matter. Mechanically, the motor is undoubtedly improved, for there are several years' time between the old Tesla three wire motor and the new German three phaser. Whether the latter shows any improvements in its principles, is a question which is anything but beyond doubt.

But in the new Dolivo von Dobrowolsky system of electric distribution, I really cannot see anything new but the mechanical construction of motors and generators. That it became so famous is, I think, entirely due to the success of the grand transmission of power over such an enormous distance as 116 miles, which cast a halo around everything that was used with this transmission, and so made the rotary motor famous; but, in reality, I think ordinary synchronous motors might just as well have been used, and would have worked just as successfully, so that the choice of the particular motor had nothing to do with the success of the power transmission.

Prof Thomson:—I should like to make some remarks upon the general subject of the paper. It is a matter to which I have given considerable thought. The subject is somewhat allied to the old Thomson-Houston arc machine. In fact, I remember long ago putting in a patent specification a machine connected so that it had not the three segment commutator but three rings, and it was rejected at the Patent Office on the ground that it was not an invention to put three rings on a three coil armature, any more than it was for any alternating current. But times have changed since then. [Laughter.] It seems to me some light would be thrown on the matter of this discussion by a few simple considerations. We will take the three coil in its simple form—symbols for it, Fig. 17. Now let us lead a wire, *J*, here, which would be a neutral wire. If we wrap that wire around a magnetic core, the effect should of course be the same or it should act the same as though these three wires, *a*, *b*, *c*, were wrapped around the magnetic core. So that if any fluctuations of magnetism were set up in this core by wrapping this neutral wire around the magnetic core, the same effect would be produced by

wrapping these three wires around the magnetic core. That would show that no fluctuation might be expected in such a system. But there is another question that comes in just here. If we look into the manner of generating; if we take our armature and put on three coils, and have a magnetic field which magnetizes this armature core with a constant number of lines, a number of lines which does not change during the rotation—it is evident, then, that whatever actions occur in these coils will be accompanied by no fluctuations of magnetism You cannot generate in a system of coils any difference of condition which is not expressed by the magnetic field in which it is generated. If the magnetic field is constant, then we have constancy of magnetism in the core. That is, we have no fluctuations. Now, it would

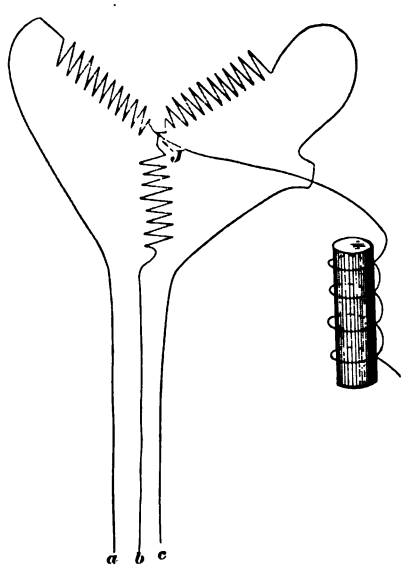

FIG. 17.

seem to me, looking at it from this standpoint, that putting another armature in connection with these corresponding terminals and having a magnetic field for it of constant strength, we would have a rotating field produced in this second armature which would result in the rotation of the armature itself—that is, the tendency to rotate the field would turn the coils backward and this would seem to indicate that under certain conditions we can, with the ordinary arrangements, produce exactly what we want, steady rotative effect without magnetic fluctuations, which would go to bear out the Ferraris idea. Now, how far the production of a rotating magnetic field in the presence of a short circuited armature may modify these conditions, I have not investigated. It does seem to me, however, that in such case the remarks of

Mr. Steinmetz are quite true, that we will have a large self-induction on a light load in such a motor and therefore a waste current, a leakage current corresponding to the leakage current in the transformer. Just how much it will be will of course depend on the general design and the proportions, and of course on the frequency of the alternations. I think the system demands that the frequencies shall be much less than we are accustomed to use in transformers for lighting That the system will have a considerable application, I have not the slightest doubt. It will have a large application. Whether the best form of it is not to place two corresponding armatures in fields, or use synchronizing machines, is a question. I should say, reasoning at the start, that this is probably the chief merit of the system. We have the possibility of getting rid of commutation on high potential transmission of power by making corresponding machines and running corresponding armatures in the fields that are excited. Of course there is a great desire to get a motor which shall enable us to get rid of this constant excitation and which will give us the power of starting under load. The generator reversed and used as a motor, demands that in starting the generator shall also start, that the two machines shall come up together. I have, indeed, taken one Thomson-Houston arc machine and put on three rings and simply delivered the current by three rings to the armature of another machine, and it would start and run up to speed, having no load on it, at least no large load. It would get into synchronous rotation when the power delivered is, after exciting the field fully, considerable. I have constructed some small machines which are almost exactly like Fig. 10. There are six coils on the armature and a field excited separately. The field, by the way, rotates and the angle covered by the field poles is about what is covered there, Fig. 10. I have no indication at all of anything wrong with these machines. They work, we think, perfectly, and so far as the heat generated in the armature goes, it is remarkably small. It is no greater, from actual experiment, than one would expect from an ordinary machine with a commutator. In fact, it is probably less. Probably more fluctuation is introduced into the magnetism by the commutation than there would be in this case by the simple use of three coils without commutation. I would say further that I have never had much confidence in some of the reasoning of Mr. Dobrowolsky in relation to this matter. I have always thought that he was a little out in his argument. He has attempted to show how, by using three phased currents, he could get polyphasal currents from them, by winding the armature in a different way. It strikes me as nothing more than spreading that coil over a certain angle of the armature surface. This, of course, would prevent sudden sharp jumps in the field and might be useful and undoubtedly is useful in the perfection of a motor working upon a closed circuit armature. These matters of discussion would undoubtedly be best

settled by a simple experimentation on the basis of what I indicated a little while ago. It may be that the very fact of the differences of composition of the wave may make a very great difference in the effect. We may not be called upon to reason upon fluctuations of magnetism due to sine curves when those sine curve currents are put in different angular positions. They may act, as in that Ferraris affair, to balance each other and oftentimes these considerations escape us.

I would say that there is another matter which probably ought to be taken into consideration. That is that a mass of iron does not always change its magnetism when you think it ought to. That is, it may hold up by its own inherent property of disregarding reversal—by hysteresis. This would tend, in fact, to hold the magnetism up rather than to allow it to suffer fluctuation. We know that in a closed circuit transformer it is necessary to put energy into the iron to cause it to drop its magnetism. It would tend to remain magnetized if we cut off the current at any portion of a wave, at least up to a certain point, and this would tend to help us out in this very matter of getting rid of fluctuations of magnetism, which of course would be a serious loss of energy in the case of large bodies of iron, the magnetism of which should be allowed to go up and down through large ranges. There is another matter, too, that comes in here. It is whether we get in the motor or in the machine a motion that is equal velocity—that is, whether it is an even pull all the way around, the sum of the magnetic lines being the same, or whether it is a jerky motion, a motion of difference of velocity. Of course, it will naturally be seen at once that unless we get a perfectly uniform flow, our efficiency could not be as high in the case of a fluctuating movement of the lines, and the lines which hesitate and then move forward and then hesitate again and then move forward, that would be only efficient in case the armature was negligible which, of course, could not be the case.

Mr. C. S. Bradley:—Did I understand Prof. Thomson to say that he thought the increase of the phasing by Dobrowolsky's plan was applicable especially to the closed armature?

Prof. Thomson:—No, I made no distinction between the kind of armatures. I simply say that Dobrowolsky's plan of multiplying the coils and connecting them up, did not differ very much, so far as I could see, from simply spreading those coils on the armature.

Mr. Bradley:—I understood you to say at the last that it applied especially to the closed circuit armature.

Prof. Thomson:—You mean by closed circuit armature, closed field and with connections taken out and around?

Mr. Bradley:—Yes.

Prof. Thomson:—No; I did not make such a distinction I should say that the closed circuit here would be just as effective for getting the polyphasal circuit, provided the coils do not cover too large an angle.

MR. BRADLEY:—It is a pretty difficult thing to make a test as to the efficiency of the different armatures. But I have always had an idea that the closed coil armature was exactly adapted to the carrying out of the three phase, because the currents can pass in more directions. There are more subdivisions than there are in the open circuit. With three open coils the position of each would determine its phase. But if it were completely closed and taken out at three points, there is a chance for two wires to operate and leave the third one *nil*, and the current has a chance to pass clear around the armature. That is, it will pass 120 degrees on one side and 240 degrees on the other side, making a multiple arc circuit. In other words, the closed coil armature carries out the plan that Dobrowolsky is trying to carry out, and does exactly what Dobrowolsky is trying to do by placing on the extra coils, without the extra coil.

PROF. THOMSON:—I think that matter is made clear by considering the movement of the armature with respect to the lines. It does not seem to me to make very much difference what the coil is, providing the two armatures—suppose we take that as the typical system—the two machines are connected similarly. The movement of lines in one will be reproduced in the other, provided of course that the order of the connections and the symmetry of the apparatus are preserved.

MR. BRADLEY:—I tested machines running that way. They were direct current machines altered to the three rings. I took them first and ran them as direct current machines. Then I had them changed and put three rings on each one and connected them up, and ran them together with the same power, and took off the same power, and I found the machines ran so nearly alike that I could not detect the difference. By the way, in your speaking of two machines, you have not at any time said motor and dynamo.

PROF. THOMSON:—I, of course, meant motor and dynamo It might interest the members for me to say that at one time, I think it was about 1882, I happened to have a three coil arc machine that had three rings on the shaft, and I discussed with my assistant. Mr. Rice, who is now superintendent of the Thomson-Houston works, this matter of connecting on three rings on two machines, and we found that the machines would work well as a means of transmitting power. But we had not facilities for building such apparatus in any quantity. However, it is an interesting reminiscence, as coming up at this time.

MR. STEINMETZ:—With regard to the spreading out of the coils on the armature, I think this makes very little if any difference in the shape of the wave, because if we had a coil of one single turn we would get a simple harmonic or sine wave where the maximum electromotive force = 1.414, the effective E. M. F. Now, only a few days ago I had occasion to draw the curve of electromotive force under exactly the opposite conditions, the

most extreme case of spreading out the coils. A smooth continuous current armature changed into a bipolar alternator by connecting two opposite commutator bars with sliding rings, and I found as ratio between maximum E. M. F. and effective E. M. F.: 1.415; that is, exactly the same value as the sine wave gives. I found a wave somewhat similar to the sine wave, slightly different, with a tendency to the shape that Dr. Pupin showed us here in Fig. 8.

PROF THOMSON:—In making the remark as to the covering of the coils, I meant a motor with a closed circuit armature—not in relation to the generation of the current.

MR. KENNELLY:—This is a paper which, I think, has to be studied from two points of view. Like all mathematical papers, it presents two different aspects. We are face to face with a condition in the art of electrical distribution which is not only somewhat novel, but is also very intricate, and consequently any assistance, even of the most elementary description, which will enable us to fathom the mysteries of this difficult system of distribution, is one which should be only too gladly welcomed, I think, at the hands of this Institute. We have presented to us in this paper, a mathematical disquisition on the very simplest and most elementary type of the three phase generator, in which it is assumed that there is no armature reaction, no hysteresis and no eddy currents. Under those conditions, just as in the corresponding ideal transformer with constant coefficients, which has been called by some one the "phantom" transformer, that we all aspire so much after but so very seldom see—the fundamental operations of this particular machine are readily capable of being analyzed by the skill that has been presented in this paper. I think that we should be content to take one subject at a time, to consider that we have arrived at a position where modifications which will certainly present themselves by disturbing influences, will ultimately resolve themselves into this fundamental type, as they are lessened and removed. This is the starting point, so to speak, from which the various roads branch from the theoretical machine to the practical machine. I think, therefore, that while the paper gives us what may be called almost a little discovery, in its way, in practical mathematics, that it should be treated for its own value on the side of a mathematical essay, and not brought into the question of practical and everyday machines. We have the very remarkable fact that although the electromotive force which is capable of being produced by a complex condition of magnetic fields can be a uniform one, and while the currents which are set up by the electromotive force cannot possibly be the prototype of the electromotive force, graphically, when there is any self-induction in the circuit, yet that under the influence of mutual induction, that prototype may be restored. That is a beautiful conception, even although it may not have a very direct bearing on the practical side of the question. It seems to me that

the practical issue of the question is not whether we can produce artificially and with considerable care a constant rotary magnetic field or a constant rotary electromotive force, for by theory we can produce any continuous curve; and any periodic wave can be established as the resultant of a number of simple harmonic waves, it only is a question of how many compounds we want. Sometimes we should want an infinite number. But however interesting that may be from the theoretical point of view, the next step in this difficult pathway which has been outlined here to-night, is not how to produce perfection, but how far differences from perfection will affect the practical result; not how necessary it may be to have a perfectly continuous field, but how far the fluctuations which will almost inevitably present themselves, will affect the efficiency and efficacy of these particular machines. At the same time, while we have to remember that the paper is a theoretical disquisition, it is also the first step towards practice, because theory follows ever slowly in the steps of practice, and it is in the direction of such a theory that we have to hope for the ultimate apprehension of all the difficulties before us.

Dr. Pupin:—The method which I have employed, as Mr. Kennelly remarked, is to go step by step from the ideal to the more and more practical, and see what the real difficulties are. Now, as long as we deal with simple harmonic waves, the question is exceedingly simple and is readily solved. The authorities on theoretical electrical engineering invariably consider the simple sine waves whenever they discuss the subject of periodically varying electric currents. Nobody has ever tackled the problem of complex harmonic waves. We really do not know what a complex harmonic wave will do. We can only guess at it. There are no quantitative relations. Now, I think that I have shown in this paper what a complex harmonic current wave will do in a particular case. I also propose to show before long what it will do in other practically important, but more complicated cases. Mr. Steinmetz points out that a combination of two simple sine waves can produce the same thing which I claim for the complex wave. But the correctness of his statement requires the employment of a homogeneous magnetic medium in which his two coils are to act. I would be very much obliged to him for the proof of the contrary.

The remark of Prof. Thomson with reference to the machine which he put on the blackboard, three coils rotating in a uniform field, has been answered in the very beginning of my paper. They will produce a constant rotary field, although the producing field, the exciting field, is constant. The relation of the coils with respect to the exciting fields, is continually varying, and the electromotive force produced in each one of those is a simple sine wave, and as long as we have simple sine waves and three phases, there will be a maximum variation of fourteen per cent.,

no matter what you do. Now wind the three wires on a piece of iron The neutral wires will produce no magnetization. The three wires, if they are wound around the same coils in the same way will also produce no magnetization. So I do not think that the experiments suggested by Prof. Thomson could give us any record of any fluctuations in the rotary field. With respect to the production of that particular wave, Mr. Steinmetz remarked that it is impossible to produce anything like it. Of course, in nature there are no discontinuities. Nature hates discontinuity, and there are no absolutely discontinuous functions. Here, you see (Fig. 8), the tangent is continually constant at all points until the corner is reached, when the tangent suddenly becomes zero. This discontinuity is, of course, physically impossible, but in practice we can come as near to it as we choose to do, if we care to take the trouble. Of course, we would employ a practically discontinuous field. That is, you are in a place where there are lines of force, and suddenly you step into a place where there are practically no lines of force. Now, absolutely, that is impossible. But we can come as near as we choose to, by simply shaping the magnetic circuit in such a way that it will be nicely rounded and allow the lines of force of the magnetizing coils to follow it without getting too much out of their way. If you do that, then there will be practical discontinuities. Now, I have an electro-magnet which will do that very nearly, where the lines of force of the magnetizing coils never deviate too suddenly from the magnetic circuit, and therefore there is very little leakage. I hope at some future time to show before the Institute, this electro-magnet. It is a very simple device. I do not say that I can produce that electro-magnet without sacrificing several practical advantages. But if it is only for experimental purposes, I do not care if I do sacrifice them. I dare say I will not rest until I get that electromotive force given in Fig. 8, plotted on paper from experimental results. But it is very difficult to do anything in New York in a hurry, because mechanicians will not attend to their orders when you wish them to, and you have to wait for them. As Mr. Kennelly remarked, I do not think it is time yet to consider what will take place when we apply this rotary field to drive a motor, because I wish to limit the discussion to the limits of the paper.

Mr. Steinmetz:—With regard to this Ferraris scheme, I only wanted to show that under certain conditions a rotary field can be produced by sine waves of electric current. Suppose you have two equal coils at right angles with each other, entirely embedded in iron. The armature of the Pacinotti, or a similar type, with a very small clearance between armature iron and field iron, so small a clearance that the air reluctance can entirely be neglected against the reluctance of the iron—then you have exactly the conditions where you get from two sine waves of 90 degrees difference of phase, a system of magnet poles which revolve with constant intensity and constant velocity.

The Secretary read the following list of Associate Members, elected by Council December 16:

Name.	Address.	Endorsed by
AYRES, BROWN,	Professor of Physics and El.ctrical Engineering, Tulane University. New Orleans, La.	E. L. Nichols. Wm E. Geyer. T. D. Lockwood.
BENJAMIN, PARK	Electrical Expert and Engineer. 32 Park Place, N. Y. City.	Ralph W. Pope. T. C. Martin. Joseph Wetzler.
BISHOP, JAMES DRAPER	Electrical Expert, John A. Roeblings' Sons Co., 345 W. 34th St., New York City	Carl Hering. T. C. Martin. Joseph Wetzler.
PARCELLE, ALBERT L.	Electrician and Inventor, 157 Washington St., Boston, Mass.	J B Cahoon. Franklin Sheble. Ralph W. Pope. T. D. Lockwood.
PIKE, CLAYTON W.	Instructor in Electrical Engineering, University of Pa., Philadelphia, Pa.	Carl Hering. E. G. Willyoung. T. C. Smith.
ROBB, WM, LISPENARD,	Professor of Physics, Trinity College. Hartford. Conn	M. I Pupin. Wm. A. Anthony. Holbrook Cushman.

Total, 6.

[Adjourned.]

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Historical Sketch of its Organization and Work.

A preliminary meeting of the founders of the American Institute of Electrical Engineers was held in New York City, April 15, 1884, in response to a call which had been very generally circulated and signed by gentlemen of prominence in electrical circles.

The following is an extract from the circular of information regarding the scope of the proposed society:

An International Electrical Exhibition is to be held in Philadelphia next autumn, to which many of the famous foreign electrical savants, engineers and manufacturers will be visitors; and it would be a lasting disgrace to American electricians if no American National Electrical Society were in existence to receive them with the honors due from their co-laborers of the United States. It is proposed to organize the American Institute of Electrical Engineers, somewhat after the model of the American Civil, Mechanical and Mining Engineers' societies or institutes.

At a subsequent meeting, held May 13, 1884, a permanent organization was effected and the following officers elected:

PRESIDENT,

DR. NORVIN GREEN.

VICE-PRESIDENTS,

PROF. A. GRAHAM BELL,
THOMAS A. EDISON,
CHARLES H. HASKINS,
PROF. CHAS. R. CROSS,
GEORGE A. HAMILTON,
FRANK L. POPE.

MANAGERS.

CHARLES F. BRUSH,
STEPHEN D. FIELD,
PROF. EDWIN J. HOUSTON,
FRANK W. JONES,
W. W. SMITH,
THEODORE N. VAIL,
WILLIAM H. ECKERT,
PROF. ELISHA GRAY,
M. L. HELLINGS,
GEORGE B. PRESCOTT,
PROF. W. P. TROWBRIDGE,
EDWARD WESTON,

TREASURER,

ROWLAND R. HAZARD.

SECRETARY,

NATHANIEL S. KEITH.

By an Act of Congress ,approved July 7, 1884, the President of the United States was authorized to appoint a scientific commission which might, in the name of the Government, conduct a National Conference of electricians in Philadelphia in the autumn of that year. The act further provided that the commission thus appointed might invite scientific men, native and foreign, to participate in the Conference, and to determine the general character and scope of its work.

In pursuance of the provisions of the act, President Arthur appointed the following distinguished scientists: Professors Henry A. Rowland, Monroe B. Snyder, George F. Barker, Simon Newcomb, C. F. Brackett, J. Willard Gibbs, Edwin J. Houston, John Trowbridge, F. C. Van Dyck and Charles A. Young, and Mr. R. A. Fisk and Dr. William H. Wahl as members of the United States Electrical Commission.

Invitations were accordingly extended by Prof. George F. Barker, Secretary of this Commission, to a large number of electricians in Europe and America, of whom about one hundred were in attendance at the Conference which assembled September 8 and adjourned September 13, 1884. The proceedings of this Conference, making a pamphlet of 183 pages, were subsequently published by the Government as a public document.

The following is an extract from the minutes of the closing session :

THE PRESIDENT :—The question has been raised as to whether we shall hold any further meetings of this Conference. It seems to me that the occasion for it will almost have passed away. The occasion of the Conference has been the Electrical Exposition of the Franklin Institute and the presence in this city of the members of the British and American Associations, so that altogether we have had a body of distinguished men, such as we could seldom get together. But some have expressed the feeling that there must be something further for the Conference to do. If any member of the Conference has anything to suggest on this point it can be presented now. Is it desired that any further meetings should be held ?

PROFESSOR ABBE :—I only express the opinion of a great many here when I say that we all agree to what has been suggested. We need very much a meeting of this kind frequently in order to exchange ideas. I am not an electrician, either professionally or practically, and it hardly becomes me to say what they need, but I can see how lively and how agreeable these meetings are. I very much hope that some means may be taken to call us together occasionally, perhaps by ourselves, perhaps by a committee.

PROFESSOR ROWLAND :—I do not supppose that we could be called together officially, except it be done within a short time. Although the law is very broad under which this meeting of the Conference has been held, we could not construe the law to make this Conference into an electrical society ; that would be impossible If within a short time a meeting is desired, I suppose that it may be called under the present law

MR. KEITH :—Pertinent to that subject I would like to say a few words. Early last spring, this exhibition being contemplated, and it being desired that the electricians should assemble themselves together in an organized body, some gentlemen in New York issued circulars to various electricians and professional men throughout the country with the stated object of forming a society. Such a society was formed last May, and the proceedings of that meeting were published, and have been circulated to some considerable extent. The membership has risen to 283. This society will meet here in Philadelphia on the 7th, 8th and 9th of October. A number of members of

the Conference are also members of this society. It is called "The American Institute of Electrical Engineers."

I shall take the liberty of sending to members of this Conference copies of the rules and minutes of the meetings held so far, so that they may become informed as to the standing of this society, and if it seems desirable to anyone that is not now a member he will be gladly welcomed into the society. We want to make it a useful institution, and make it large and financially strong as well as intellectually strong; if not financially strong it certainly will not be intellectually, because finances support intelligence.

The Directors of the International Electrical Exhibition, held in Philadelphia in 1884, kindly tendered to the Institute the free use of rooms in the Exhibition Building, which were accepted by Council, and the rooms were opened to members September 2d, and continued until October 11th, during which time they were visited by a great many members of the Institute and by several foreign electricians

On October 7th and 8th the first meeting of the Institute for the reading and discussion of professional papers was held at the Continental Hotel and the Exhibition Rooms, at which eleven papers were read, and upon invitation of the Franklin Institute the Exhibition was inspected by the members in a body.

The subsequent history of the Institute is well known. After the holding of two or more annual meetings in New York, records of which have been printed in the TRANSACTIONS, the experiment of monthly meetings was begun, in accordance with suggestions made at the annual meeting in May, 1885, by Acting Secretary Martin, followed in May, 1886, by his successor, as will be seen by the following extract from the Secretary's report, submitted to the Institute:

The importance of the various electrical problems which are continually being brought to public notice points to the advisability of holding monthly meetings for the discussion of current topics. There are in New York and vicinity a sufficient number of members to make such meetings successful, especially if those who are capable and have had ample electrical experience will perform their duty in the preparation of suitable papers for presentation on such occasions. I can see no reason why such a gathering might not be held monthly in conjunction with the regular meetings of Council under existing arrangements. It also appears probable that if suitable arrangements were made for reporting the proceedings of these meetings many important electrical papers would be brought before the Institute which are now presented to other societies, although perhaps entirely foreign to their special field. This is largely the result of a popular interest in electrical researches now existing, which we should avail ourselves of to stimulate the growth of the Institute.

With the inauguration of such a policy as I have outlined, your Secretary will feel far better satisfied with his work, and the existence of the Institute will gradually become known, and properly appreciated by those for whose benefit it was founded.

The first of these meetings, which may be said to have awakened new interest in the welfare of the Institute, was held June 8, 1886, in the Mills Building, 15 Broad street, and was preceded by an informal dinner. The meeting was called to order by President Franklin L. Pope, who in announcing the subject of dis-

cussion, "Incandescent Lighting from Central Stations," made the following remark:

It gives me very much pleasure to see so large an attendance on this occasion, the first of our monthly meetings, and it affords us excellent encouragement as to what we may expect in the future.

In the autumn of the same year (1886) these monthly meetings were resumed and have continued regularly since that time, being omitted during the summer months. These monthly meetings have been held in New York City, for the reason that a sufficient number of members can always be depended upon to prepare and discuss professional papers.

The General Meeting of 1890 was held at Boston, May 21st, the membership in that city and vicinity having become sufficiently large to warrant its success. The occasion will long be remembered by those who were present, and it appears probable that hereafter, by the judicious selection of location, the May meetings of the Institute may be held each year at different cities in the country.

Having become fairly established as the national electrical organization of America, it was deemed proper that the Institute should take active steps towards identifying itself with international work, thereby securing proper recognition for the important researches of Americans, which had hitherto been ignored, simply because no organized body representing this country had participated in the International Congresses of Europe.

Such a Congress was held in Paris in 1889, in connection with the International Exposition. As it was then supposed that a World's Fair would be held in America in 1892, the Council suggested that an International Congress should be convened at that time. With this purpose in view, the following delegates were appointed who attended the Paris Congress:

THOMAS A. EDISON, CARL HERING,
JOSEPH WETZLER, E. WILBUR RICE, JR.,
NIKOLA TESLA.

Their presence at the Congress was recorded in the official proceedings.

In August, 1889, the following committee was appointed by President Elihu Thomson to make preparations for the proposed International Congress in America:

EDWARD WESTON, S. S. WHEELER,
G. A. HAMILTON, HENRY MORTON,
CHAS R. CROSS, C. H. HASKINS,
R. W. POPE, W. A. ANTHONY,
E. J. HOUSTON, F. L. POPE,
T. C. MARTIN, T. D. LOCKWOOD,
GEORGE M. PHELPS, C. O. MAILLOUX,
CARL HERING.

The postponement of the date of the World's Fair until 1893, and the holding of an International Congress at Frankfort in 1891, rendered it unnecessary to take any further steps in this direction, beyond taking advantage of the coming Frankfort Congress to renew the invitation to hold an International Congress in America.

On December 3, 1889, the Council appointed a Committee of Eleven "to formulate and submit for approval a standard wiring table for lighting and power purposes." This committee submitted a preliminary report which was discussed at the monthly meeting of the Institute, September 16, 1890, when certain amendments were suggested which were embodied in a revised report, submitted November 18, 1890, which was ordered printed, that distant members might be afforded an opportunity to criticize it.

At the meeting of Council, held June 17, 1890, the following resolutions were referred to a meeting of the Institute and came up for action September 16, 1890:

Whereas, It has been the custom in the nomenclature of electrical units to perpetuate the names of men who have contributed most to electrical science; and,

Whereas, In the names thus far adopted the eminent services of Amercans have not been recognized; therefore,

Resolved, That in the opinion of the American Institute of Electrical Engineers a just distribution of the honors thus bestowed necessitates a recognition of the splendid contributions to electrical science of one or both of America's great electricians—Benjamin Franklin and Joseph Henry; and,

Resolved, That this Institute will gladly co-operate with other bodies in this country and abroad to secure the general adoption of these names for electrical units; and,

Resolved, That the name of Henry should be given to the practical unit of self-induction, since he was the discoverer and greatest investigator of this phenomenon, and because this unit at present is called a quadrant, which is merely a numerical value and not a suitable name.

Resolved, That this Institute recommend to electrical societies and electrical engineers the general use of the name Henry for the unit of induction as being the quickest and surest way to secure its final adoption.

These resolutions were subsequently amended in accordance with the following suggestion:

MR. F. L. POPE:—It seems to me that the ultimate disposition we ought to make of this matter is to refer it to the International Electrical Conference, with our recommendation that the name "Henry" be given to the practical unit of self-induction and our recommendation of a certain value. It does not seem to me that we can settle this thing all by ourselves. The rest would like at least to be consulted about it. I think if it were referred to them in that way there is very little doubt they could adopt it.

Upon the receipt of an invitation extended by the management of the Frankfort International Congress of 1891 to the American Institute of Electrical Engineers, to take part in its deliberations, the Institute, at the general meeting May 21st, authorized the Chair to appoint five delegates to attend the Congress, and the following delegation was subsequently appointed:

MR. CARL HERING, Chairman,
DR. EDWARD L. NICHOLS, MR. NIKOLA TESLA,
PROF. RICHARD O. HEINRICH, MR. LUDWIG GUTMANN.

In his report to the Council, at its meeting October 27, 1891, Vice-President Carl Hering, Chairman of the Delegation, submitted his official report, which was accepted and ordered printed in the TRANSACTIONS. In this report it is stated that the delegation concluded to bring up two or three matters which had occupied the attention of the Institute, namely:

The adoption of the name "Henry" and a normal value for the resistance of copper, and in addition suggested the adoption of a practical unit of magnetism. These they concluded were the most important questions to be brought up at this Congress. They prepared a short announcement of these propositions (in German) in the name of the American Institute of Electrical Engineers and had it printed in full in one of the daily programmes, which were distributed among all the members; a place was also assigned to this matter at the second general session, September 10th, as announced on this notice, which appeared two days before.

The substance of the propositions, as announced, was as follows:

1. That the American Institute of Electrical Engineers had adopted the name "Henry" for the name of the practical unit of induction, and that we propose that this Congress formally adopt the name. This was followed by an explanation of the value of this unit, why this name was preferred to the word "quadrant," and why it was named after Prof. Henry.

2. That this Congress ought to adopt a name and value for a practical unit of the intensity of magnetism. To open a discussion we proposed the name "Gauss" and a value of 1,000 lines per square centimeter, or 10.3 absolute units.

3. That it would be desirable to adopt a normal value for the resistance of copper to facilitate relative comparisons for commercial purposes. To this we added that we wished merely to agitate this question here, in order that any preliminary tests to determine such a value may be made between now and the next Congress, so as to facilitate its adoption at that Congress.

4. The announcement closed with an invitation to the members of this Congress, on the part of the American Institute of Electrical Engineers to participate in the next International Congress, to be held under the auspices of this Institute in Chicago, in 1893, (at that time your delegates were not informed that this coming Congress was in the hands of a committee of the World's Fair).

Although these propositions were not adopted, chiefly on account of lack of time, the effect of making them has prepared the way for their adoption at the next Congress. Mr. Hering suggests that the Institute profit by this experience, and that, as the leading American electrical society, it ought to take the lead in the matter of establishing and naming practical magnetic units and other similar international matters, by considering the subject now at their meetings, making propositions, publishing them in domestic and foreign journals, inviting the aid and criticism of foreign electricians and societies, and in this way prepare for

their final settlement at the next Congress. A Congress is hardly the place to start a discussion on such matters; it is the place to finally settle a discussion which should precede it in the societies and journals of the leading nations.

The adjournment of the Frankfort Congress, to meet in Chicago in 1893, was considered as settling the fact that an International Electrical Congress was to be held at that time, and it therefore seemed proper that the Institute should proceed with its preparatory work with this end in view, it having had the advantage of experience in the deliberations of two such bodies, while a large number of its members had also participated in the National Electrical Conference at Philadelphia in 1884, already referred to. The Committee on the International Electrical Congress of 1893 accordingly met October 27th, 1891, upon call of the Secretary of the Institute, and after organizing, appointed additional members and a sub-committee, so that it now stands as follows:

T. Commerford Martin, Chairman,
150 Broadway, New York City.

Ralph W. Pope, Secretary,
12 West 31st St., New York City.

Prof. W. A. Anthony,
Prof. Alex. Graham Bell,
Prof. Francis B. Crocker,
Prof. Charles R. Cross,
Dr. William E. Geyer,
Mr. Ludwig Gutmann,
Mr. George A. Hamilton,
Col. Chas. H. Haskins,
Mr. Carl Hering,
Prof. Edwin J. Houston,
Mr. Thomas D. Lockwood,
Mr. C. O. Mailloux,
Prof. Henry Morton,
Dr. Edward L. Nichols,
Mr. George M. Phelps,
Mr. Franklin L. Pope,
Mr. Nikola Tesla,
Prof. Elihu Thomson,
Mr. Edward Weston,
Dr. Schuyler S. Wheeler,
Mr. A. E. Kennelly.

Sub-Committee on Provisional Programme,

Mr. Carl Hering, Prof. W. A. Anthony,
Mr. A. E. Kennelly.

In addition to the legislative work as outlined, a paper was read before the Institute at its meeting, December 16, 1890, upon "Inductance and Its Proposed Unit the Henry," by Mr. A. E. Kennelly, which was discussed at the following meeting, January 20, 1891, together with the report of the Committee on the Valuation of the Henry, all of which appear in the numbers of the Transactions for January and February, 1891.

Since the above preparations for an International Electrical Congress were undertaken by the Institute, the organization of the World's Electrical Congress in connection with the World's Columbian Exposition has been placed in the hands of the Electrical Congress Committee of the World's Congress Auxiliary, of which Dr. Elisha Gray is Chairman, and the authorities of the Institute are working in harmony with it, as is shown in the following resolution, passed by the Institute's Congress Committee December 16th, 1891, and approved at a special meeting of Council held December 23rd, 1891.

Resolved, That the American Institute of Electrical Engineers, having already taken action during the past three years, by correspondence and otherwise, towards the holding of an International Electrical Congress in connection with the Columbian World's Fair, hereby expresses its desire and intention to co-operate, by all means in its power, with the World's Congress Auxiliary of the World's Columbian Exposition, through its Electrical Congress Committee, in furthering the gathering of such a Congress at Chicago in 1893, and in making it a successful and worthy representation of the best electrical science and practice in all parts of the world.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

CATALOGUE OF MEMBERS.

JANUARY 1ST, 1892.

HONORARY MEMBERS.

Name.	Address.	Date of Membership.
FARMER, PROF. MOSES G.	Electrician and Inventor, Eliot, Me.	A April 15, 1884 H.M. Oct. 21, '90
GREEN, DR. NORVIN	(*Past President*). Pres't Western Union Telegraph Co. P. O. Box 3393, 195 Broadway, New York.	A April 15, 1884 M Oct 21, 1884 H.M. Dec. 3, 1889
PREECE, WM. H. *F. R. S.*	Electrician, General Post Office, London, Eng. Residence, Gothic Lodge, Wimbledon.	H.M. Oct. 21, 1884

MEMBERS.

ACHESON, EDW. G.	President, The Monongahela Electric Light Co., Monongahela City, Pa.	A Jan. 3, 1888 M May 1, 1888
AHEARN, T.	Ahearn & Soper, Electrical Supplies, Ottawa, Ont.	A July 12, 1887 M Sept. 6, 1887
ANTHONY, PROF. W. A.	(*Past President.*) Consult'g Electrician. The Mather Electric Co., Manchester, Conn.	A Dec. 9, 1884 M Jan. 6, 1885
BATCHELOR, CHAS.	Electrical Engineer, 33 West Twenty-fifth St., New York.	A June 8, 1887 M July 12, 1887
BATES, J. H.	Moore Electrical Manufacturing Co., 652 Hudson St., New York City.	A Sept. 6, 1887 M Oct. 1, 1889
BATES, MAURICE E.	Manager, Western Electric Construction Co., 125 West 6th St., Kansas City, Mo.	A Aug. 6, 1889 M Oct. 1, 1889
BAUER, A. H.	Electrical Engineer, Pullman Palace Car Co., Room 307, Pullman Building, Chicago, Ill.	A Feb. 7 1890 M Apr. 21, 1891
BAYLY, B.	Surveyor and District Engineer, Western District Colonial Gov't. Cape Town, Cape of Good Hope, Africa.	A Oct. 4, 1887 M Dec. 6. 1887
BELL, PROF. A. GRAHAM	(*President.*) 1336 Nineteenth St., Washington, D. C.	A April 15, 1884 M Oct. 21, 1884
BELL, DR. LOUIS,	(*Manager.*) Editor *Electrical World*, 167 Times Building, P. O. Box 3332, N. Y.	A May 20, 1890 M June 17, 1890
BERNARD, EDGAR G.	Electrical Engineer, E. G. Bernard & Co., Hall Building, Troy, N. Y.	A Jan. 5, 1886 M July 12, 1887

Name.	Address.	Date of Membership.
BINNEY, HAROLD	Assistant in the Practice of Patent Law, with Gilbert M. Plympton, 245 Broadway, New York City.	A Sept. 16, 1890 M Dec. 16, 1890
BIRDSALL, E. T. *M. E.*	Electrical Engineer, 115 Broadway, New York.	A June 8, 1887 M Nov. 1, 1887
BLAKE, EDWARD	361 Temple Street, New Haven, Conn.	A May 24, 1887 M Sept. 3, 1889
BLAKE, FRANCIS	Auburndale, Mass.	A Sept. 3, 1889 M Oct. 1, 1889
BLODGETT, GEO. W.	Electrical Engineer B. & A. R. R. and Consulting Electrician, Boston, Mass.	A July 12, 1887 M Sept. 6, 1887
BOSCH, ADAM	Sup't, Fire Alarm Telegraph, Newark, N. J.	A April 15, 1884 M Jan. 6, 1885
BOYNTON, EDWARD C.	Thomson-Houston Electric Co., Residence, 24 Chase St., Lynn, Mass.	A Aug. 6, 1889 M Nov. 24, 1891
BRADLEY, CHAS. S.	Electrical Engineer, Wayne Hotel, Fort Wayne, Ind.	A May 24, 1887 M Dec. 6, 1887
BROOKS, MORGAN	Secretary and Treasurer, The Electrical Engineering & Supply Co., 413 Jackson St., and 529 Holly Ave., St. Paul, Minn.	A May 20, 1890 M June 17, 1890
BROWN, J. STANFORD	Consulting Electrical Engineer, 611-12 Electrical Exchange Building, New York, and 63 Livingston St., Brooklyn, N. Y.	A Sept. 6, 1887 M Nov. 1, 1887
BRUSH, CHAS. F.	Electrical Engineer, 956 Euclid Ave, Cleveland, O.	A April 15, 1884 M Oct. 21, 1884
BYLLESBY, HENRY M.	Northwest Thomson-Houston Electric Co., 403 Sibley St., St. Paul, Minn.	A Sept. 7, 1888 M Oct. 2, 1888
CAHOON, JAS. B.	Electrical Engineer, Thomson-Houston Electric Co., 68 Baker St., Lynn, Mass.	A June 17, 1890 M May 19, 1891
CARROLL, LEIGH	President and General Manager, The Edison Electric Illuminating Co., Birmingham, Ala.	A Oct. 1, 1889 M Nov. 12, 1889
CHAMBERLAIN, J. C.	(*Manager.*) Electrical Engineer, 136 Liberty St., and 135 East 18th St., New York.	A Dec. 6, 1887 M Jan. 3, 1888
CHEEVER, CHAS. A.	13 Park Row, New York.	A April 15, 1884 M Oct. 21, 1884
CHURCH, WM. LEE	Member of Westinghouse, Church, Kerr & Co., Engineers, 620 Atlantic Ave., Boston, Mass.	A April 5, 1887 M May 17, 1887
CLARK, ERNEST P.	Electrical Engineer, Clark Electric Co., 192 Broadway, New York.	A Jan. 8, 1887 M Nov. 1, 1887
CLARKE, CHAS. L.	Mechanical and Electrical Engineer, 55 Liberty St., New York.	A April 15, 1884 M Jan. 6, 1885
COLBY, EDWARD A	Lock Box 313. Newark, N. J.	A April 2, 1889 M May 7, 1889
CONDICT, G. HERBERT	General Manager, The Electric Car Co. of America, 4720 Green St., Germantown, Pa.	A July 12, 1887 M Sept. 6, 1887
COTHREN, WM. H.	Edison General Electric Co., Rialto Building. Chicago, Ill	A Aug. 6, 1889 M Oct. 1, 1889

Name.	Address.	Date of Membership.
COWLES, ALFRED H.	Technical Adviser to the Cowles Smelting and Aluminum Co., 656 Prospect St., Cleveland, O.	A Mar. 5, 1889 M May 7, 1889
CROCKER, FRANCIS B.	(*Vice-President.*) Instructor in Electrical Engineering, School of Mines Columbia College, and 54 W. 21st Street, New York.	A May 24, 1887 M April 2, 1889
CROSS, PROF. CHAS. R.	Thayer Professor of Physics, and Director of the Rogers Laboratory, Mass. Institute of Technology, Boston, Mass.	A April 15, 1884 M Oct. 21, 1884
CURTISS, GEORGE F.	Electrician, Thomson-Houston Electric Co., Lynn, Mass.	A April 2, 1889 M Nov. 24, 1891
CUTTER, GEORGE	Dealer in Electrical Supplies, 333 The "Rookery," Chicago, Ills.	A June 17, 1890 M May 19, 1891
CUTTRISS, CHAS.	Electrician. The Commercial Cable Co., 1 Broad St., New York.	A Nov. 1, 1887 M Dec. 6, 1887
DAFT, LEO	Electrician, Seattle, Wash.	A Dec. 9, 1884 M Jan. 6, 1885
DANVERS, ALAN	Managing Director and Chief Electrician, The Anglo-Portugese Telephone Co., L't'd. 60 Travessa Santa Justa, Lisbon, Port'l.	A Nov. 1, 1887 M Sept. 3, 1889
DANVERS, ERNESTO	Electrical Engineer, Casilla 74, Rosario, Argentine Republic.	A Jan. 3, 1888 M May 1, 1888
DAVIES, JOHN E.	Professor of Physics, University of Wisconsin, 523 Carroll St Madison, Wis.	A Jan. 7, 1890 M Mar. 18, 1890
DAVIS, CHARLES H.	Consulting Electrical Engineer, 120 Broadway, New York.	A Mar. 18, 1890 M June 17, 1890
DELAFIELD, A. FLOYD, *Ph.D.*	Electrical Engineer, Noroton, Conn.	A May 7, 1889 M Oct. 1, 1889
DELANY, PATRICK BERNARD, (*Manager*).	Inventor, South Orange, N. J.	A April 15, 1884 M Nov. 24, 1891
DICKENSON, SAMUEL S.	Sup't, Commercial Cable Co., Hazel-Hill, Guysborough Co., N.S.	A Mar. 6, 1888 M Oct. 1, 1889
DIEHL, PHILIP	Inventor, Singer Sewing Machine Co., 508 Morris Ave., Elizabeth, N. J.	A April 15, 1884 M Dec. 9, 1884
D'INFREVILLE, GEORGES	Electrical Engineer and Expert 110 Liberty St., New York.	A Nov. 1, 1887 M Dec. 6, 1887
DOIJER, H.	Electrical Engineer, 24 Keppel St., Russell Square, W. C. London, Eng.	A Jan. 7, 1890 M Mar. 18, 1890
DONNER, WILLIAM H.	Sterns Mfg. Co., Erie, Pa.	A Nov. 18, 1890 M Dec. 16, 1890
DUDLEY, CHARLES B.	Chemist and Scientific Expert, Penn. R. R. Co, 1219 Twelfth Ave., Altoona, Pa.	A Oct. 1, 1889 M Nov. 12, 1889
DUNCAN, DR. LOUIS	(*Vice-President*). Johns Hopkins University, Baltimore, Md., Firm of Sprague, Duncan & Hutchinson, 15 Wall St., New York City.	A July 12, 1887 M Sept. 6, 1887
DYER, R. N.	Patent Attorney, 36 Wall St., New York.	A July 12, 1887 M Sept. 6, 1887
EDISON, THOMAS A.	Mechanician and Inventor, Orange, N. J.	A April 15, 1884 M Oct. 21, 1884

Name.	Address.	Date of Membership.
EUSTIS, HERBERT H.	President and Electrician, Eastern Electric Cable Co., 61 Hampshire St., Boston, Mass.	A April 15, 1890 M Nov. 24, 1891
FARNHAM, ISAIAH H.	Electrician for the N. E. Telephone & Telegraph Co., 50 Pearl St., Boston, Mass.	A June 8, 1887 M July 12, 1887
FESSENDEN, REGINALD A.	Assistant Electrician, U. S. Electric Light Co., Newark, N. J.	A Oct. 21, 1890 M Dec. 16, 1890
FIELD, C. J.	President and General Manager Field Engineering Co., 143 Liberty St., New York.	A June 8, 1887 M Nov. 1, 1887
FIELD, STEPHEN D.	Electrician and Inventor, Stockbridge, Mass.	A April 15, 1884 M Oct. 21, 1884
FITCH, D. H.	President, The Fitch Battery and Electric Co., Oneida, N. Y.	A Sept. 3, 1889 M Oct. 1, 1889
FLEMING, WILFRID H.	Trask Avenue, Bayonne City, N. J.	A Dec. 6, 1887 M Jan. 3, 1888
FOSTER, HORATIO A.	(*Manager.*) Electrical Engineer, 548 E. 164th St., New York.	A June 8, 1887 M Sept. 6, 1887
FREEMAN, DR. FRANK L.	Attorney-at-Law, Solicitor of Patents, Electrical Expert, 931 F St., Washington, D. C.	A May 7, 1889 M Sept. 3, 1889
GARRATT, ALLAN V.	President, Spokane and University Heights Street Railway Co., Spokane Falls, Wash.	A April 2, 1889 M May 7, 1889
GARVER, M. M.	(Present address unknown.)	A July 10, 1888 M Sept 3, 1889
GEYER, DR. WM. E.	(*Manager*). Stevens Institute of Technology, Hoboken, N. J.	A June 5, 1888 M Sept. 7, 1888
GRISCOM, WM. W.	Haverford College P. O., Montgomery Co., Pa. President, The Electro Dynamic Co., 224 Carter St., Philadelphia.	A June 5, 1888 M Mar. 18, 1890
HALL, CLAYTON C.	Civil Engineer, 810 Park Ave., Baltimore, Md.	A April 15, 1884 M Oct. 21, 1884
HAMBLET, JAMES	(*Manager.*) Manager Time Service, W. U. Tel. Co., 195 Broadway, P. O. Box 3393, New York.	A Nov. 1, 1887 M Dec. 6, 1887
HAMILTON, GEO. A.	Electrician, Western Electric Co., 22 Thames cor. Greenwich St., New York and 532 Morris Ave., Elizabeth, N. J.	A April 15, 1884 M Oct. 21, 1884
HAMMER, W. J.	(*Vice-President.*) Consulting and Supervising Electrical Engineer, 533 Temple Court, N. Y., and 23 Rowland St., Newark, N. J.	A June 8, 1887 M July 12, 1887
HASKINS, CHARLES H.	Electrician, 80 Broadway, New York.	A April 15, 1884 M Oct. 21, 1884
HAYES, HAMMOND V.	Electrician, American Bell Telephone Co., 127 Purchase St., Boston, Mass.	A Nov. 12, 1889 M Mar. 18, 1890
HAYNES, F. T. J.	Divisional Telegraph Engineer, Great Western Railway, Taunton, Eng.	A Dec. 6, 1886 M Jan. 3, 1887
HAZARD, COL. ROWLAND R.	N. Y. District Railway Co., 120 Broadway, New York.	A April 15, 1884 M Oct. 21, 1884

Name.	Address.	Date of Membership.
HERING, CARL	(*Vice-President.*) Consulting Electrical Engineer, 927 Chestnut St., Philadelphia, Pa.	A Jan. 3, 1888 M June 5, 1888
HERZOG, DR. F. BENEDICT	(*Manager*) President Herzog Teleseme Co., 30 Broad St., New York.	A May 24, 1887 M July 12, 1887
HIGGINS, EDWARD E.	Electrical Engineer, Genera lManager, The Short Electric Railway Co. Cleveland, O.	A June 8, 1887 M July 12, 1887
HOUSTON, PROF. EDWIN J.	Prof. of Physics, Franklin Inst, Prof of Physics and Physical Geography, Central High School, 1809 Spring Garden St., Philadelphia, Pa.	A April 15, 1884 M Oct. 21, 1884
HOWELL, JOHN W.	Electrician, Edison Lamp Works, Harrison N. J.	A July 12, 1887 M June 5, 1888
HOWELL, WILSON S.	Electrical Expert, Edison Lamp Works, Harrison, N. J.	A Sept. 3, 1889 M Mar. 18, 1890
HUNTER, RUDOLPH M.	Mechanical and Elect'al Engineer, 926 Walnut St., Philadelphia, Pa.	A July 13, 1886 M May 17, 1887
HUTCHINSON, DR. CARY T.	Electrical Engineer, 56 West 25th St., Firm of Sprague, Duncan & Hutchinson, 15 Wall St., New York City.	A Feb. 7, 1890 M Dec. 16, 1890
HYDE, JEROME W.	Springfield, Mass.	A June 8, 1887 M Nov. 1, 1887
JACKSON, DUGALD C.	Professor of Electrical Engineering University of Wisconsin, Madison, Wis.	A May 3, 1887 M June 17, 1890
JACKSON, FRANCIS E.	With Edison Lamp Works, Harrison N. J.	A Jan. 3, 1888 M June 17, 1890
JANNUS, FRANKLAND	Solicitor of Patents, 928-30 F. St Washington, D. C.	A Nov. 12, 1889 M Mar. 18, 1890
JENKS, W. J.	Technical Department, Edison, General Electric Co., 44 Broad Street, Box 3067, New York.	A June 8, 1887 M Nov. 1, 1887
JONES, FRANCIS W.	Assistant Gen'l-Manager and Electrician, Postal-Telegraph Cable Co., 5 Dey St., New York.	A April 15, 1884 M Oct. 21, 1884
KNOWLES, E. R.	Chief Electrician, The Schuyler Electric Company, Middletown, Conn.	A June 8, 1887 M July 12, 1887
KNUDSON, A. A.	Manager, Eastern Electric Co., St. John. N. B.	A Dec. 6, 1887 M Jan. 3, 1888
LANGE, PHILIP A.	Superintendent of Detail Department, Westinghouse Electric Co., Pittsburgh, Pa.	A Mar. 6, 1888 M June 5, 1888
LANGTON, JOHN	General Manager, Canadian Edison M'f'g Co., Peterboro. Ont.	A Mar. 6, 1888 M June 5, 1888
LATTIG, J. W.	Sup't and Treasurer, National Switch and Target Co. South Easton, Pa	A June 8, 1887 M July 12, 1887
LAWSON, A. J.	Electrical Engineer and Contractor, Box 1009. Montreal, P. Q.	A Mar. 18, 1890 M June 17, 1890
LEONARD, H. WARD	(*Manager*). General Manager, The H. Ward Leonard Co., 136 Liberty St. New York.	A July 12, 1887 M Sept. 6, 1887
LEONARD, M. B.	Electrical Engineer, and Supt. of Telegraph, Chesapeake & Ohio R'y. Co., Richmond, Va.	A Nov. 6, 1886 M May 1, 1888

Name.	Address.	Date of Membership.
LIEB, JOHN W., JR.	Chief Engineer, Società Generale Italiana di Elettricita (Sistema Edison). Via S. Radigonda N. 4, Milan, Italy.	A Sept. 6, 1887 M Nov. 1, 1887
LOCKWOOD, THOMAS D.	(*Vice-President.*) Electrical Engineer. and Advisory Electrician, P. O. Drawer 2, Boston, Mass.	A April 15, 1884 M Oct. 21, 1884
LYNE, LEWIS F.	Mechanical and Elect'al Engineer, 307 Grove St., Jersey City, N. J.	A Jan. 3, 1888 M June 5, 1888
MAILLOUX, C. O.	Consulting Electrical Engineer, 32 Liberty St., New York.	A April 15, 1884 M Oct. 21, 1884
MARKS, WILLIAM DENNIS,	*Ph. B., C. E.* Edison Electric Light Co., Philadelphia, Pa.	A Feb. 7, 1888 M May 1, 1888
MARSHALL, J. T.	Inspector of Lamp Manufacture, Edison Lamp Works, Harrison, N. J.	A Oct. 1, 1889 M Nov. 12, 1889
MAVER, WILLIAM, JR.	Electrical Expert, 31 Nassau St., New York.	A July 12, 1887 M Apr. 21, 1891
MAYNARD, GEO. C.	Electrical Engineer, 1409 New York Ave., Washington, D. C.	A April 15, 1884 M Dec. 9. 1888
MCCAY, H. K.	Electrician, 4426 Sansom St., N. Philadelphia, Pa.	A Sept. 16, 1890 M May 19, 1891
MILLIS, JOHN	Lieutenant of Engineers, U. S. Army, P.O. Drawer 432. New Orleans, La.	A July 7, 1884 M Mar. 3, 1885
MILLS, FRANK P.	Superintendent Cleveland Iron Mining Co., Ishpeming, Mich.	A Jan. 6, 1885 M Mar. 3, 1885
NICHOLS, DR. EDWARD L.	Professor of Physics, at Cornell University, Ithaca, N. Y.	A Oct. 4, 1887 M Dec. 6, 1887
PAINE, SIDNEY B.	Electrical Constructor, member of firm "Paine & Francis," 25 Otis St., Boston, Mass.	A June 8, 1887 M Nov. 1, 1887
PAINE, F. B. H.	Edison General Electric Co., Fleischner Building, Portland, Oregon.	A Dec. 16, 1890 M Nov. 25, 1891
PARKS, C. WELLMAN	Electrician, 1825 Fifth Ave., Troy, N. Y	A July 12, 1887 M May 1, 1888
PARSHALL, H. F.	Electrical Engineer, Edison General Electric Co., Schenectady, N. Y.	A Sept. 7, 1888 M Mar. 18, 1890
PATTEN, F. JARVIS,	Electrical Engineer, 45 Broadway, New York.	A Sept. 6, 1887 M Nov. 1, 1888
PATTISON, FRANK A.	Firm of Pattison Bros., Consulting and Constructing Electrical Engineers, 135 Broadway, New York City.	A Sept. 22, 1891 M Dec. 16, 1891
PERRINE, FREDERIC A. C.	Superintendent Insulated Wire Dep't, John A. Roebling's Sons Co., 137 E. State St., Trenton, N. J.	A Sept. 16, 1890 M Dec. 16, 1890
PFANNKUCHE, GUSTAV	Engineer and Inventor, 655 Wilson Ave., Cleveland, O.	A Feb. 7, 1888 M May 1, 1888
PHELPS, GEO. M.	(*Treasurer*). President, *Electrical Engineer*, 150 Broadway, New York.	A April 15, 1884 M Oct. 21, 1884
PICKERNELL, F. A.	Sup't. of Equipment, Amer. Tel. & Tel. Co., 18 Cortlandt St. N. Y.	A Feb. 7, 1890 M Mar. 18, 1890

Name.	Address.	Date of Membership.
POPE, FRANKLIN L.	(*Past President*). Consulting Electrical Engineer and Expert, 15 Wall St., N. Y. Residence, Elizabeth, N. J.	A April 15, 1884 M Oct. 21, 1884
PORTER, J. F.	29 Broadway, New York City.	A Sept. 6, 1887 M Nov. 1, 1887
PRATT, ROBERT J.	Electrician. Treas. and Mgr. Electric Mfg. Co. and Gas Engine Co., Greenbush, N. Y.	A July 12, 1887 M Sept. 6, 1887
PRESCOTT, GEO. B., JR.	Electrical Engineer. Pittsfield, Mass.	A July 12, 1887 M Nov. 1, 1887
RAYMOND, CHAS. W.	Civil and Electrical Engineer, Merrimac Consolidated Mining Co., Platoro, Conejos Co., Col.	A June 8, 1887 M May 17, 1887
RECKENZAUN, ANTHONY	Electrical Engineer, 7 Albert Terrace, Hemberton Road, Stockwell, London, S. W. England.	A Nov. 1, 1887 M Dec. 6, 1887
RECKENZAUN, FREDERICK,	Electrician, Box 225, West Hoboken, N. J.	A Mar. 6, 1888 M June 5, 1888
RICE, E. WILBUR, JR.	General Superintendent and Consulting Electrician, Thomson-Houston Elec. Co., Lynn, Mass.	A Dec. 6, 1887 M Jan. 3, 1888
RIES, ELIAS E.	Electrician and Electrical Engineer, 430 South Broadway, Baltimore, Md.	A July 12, 1887 M Sept. 6, 1887
ROBERTS, E. P.	Electrician and Sup't, Swan Lamp Co., Belden St., Cleveland, O.	A Jan. 6, 1885 M Feb. 3, 1885
ROHRER, ALBERT L.	Electrical Engineer, with Thomson-Houston Electric Co., Lynn, Mass.	A Nov. 1, 1887 M May 1, 1888
SALOMONS, *Sir* DAVID LIONEL, *Bart. M. A.*,	Engineer and Barrister, Broomhill, Tunbridge Wells, Kent, and 49 Grosvenor St., London, W., England.	A Feb. 7, 1888 M May 1, 1888
SCHULZE-BERGE, FRANZ	44 Monroe Place, Brooklyn, N. Y.	A Nov. 12, 1889 M Mar. 18, 1890
SHALLENBERGER, O. B.	Electrician, Westinghouse Electric Co , Pittsburgh, Pa.	A Sept. 7, 1888 M Dec. 4, 1888
SHELDON, SAMUEL, *A. M. Ph. D.*	Professor of Physics and Electrical Engineering, Polytechnic Institute, 170 State St., Brooklyn, N Y.	A Dec. 16, 1890 M Oct. 27, 1891
SHEPARD, WM. E.	Lincoln St. Ry. Co., Lincoln, Neb.	A Feb. 7, 1890 M Mar. 18, 1890
SLATER, HENRY B.	Manager and Sup't, The Canon City Electric Light and Power Co., Canon City Col.	A April 15, 1884 M Dec. 9, 1884
SMITH, JESSE M.	Consulting Electrical Engineer and Expert in Patent Causes, 36 Moffatt Block, Detroit, Mich.	A April 15, 1884 M June 26, 1891
SMITH, T. CARPENTER	Partner in fir n of M. R. Muckle Jr. & Co., 212 Drexel Building, Philadelphia, Pa.	A Oct. 27, 1891 M Dec. 16, 1891
STANDFORD, WILLIAM	Ass't Sup't Telegraphs, Colonial Gov't, Cape Town, Cape of Good Hope, Africa.	A Oct. 4, 1887 M Dec. 6, 1887
STEBBINS, THEODORE	Electric Railway Inspector, Thomson-Houston Elec. Co , 620 Atlantic Ave., Boston, Mass.	A July 9, 1889 M June 17, 1890

Name.	Address.	Date of Election.
STEINMETZ, CHARLES P.	Electrician, Osterheld & Eickemeyer, Yonkers, N. Y.	A Mar. 18, 1890 M April 21, 1891
STIERINGER, LUTHER	Electrical Expert, 1873 Lexington Ave., New York.	A June 8, 1887 M Nov, 1, 1887
TAINTOR, GILES	Assistant Electrician, N. E. Telephone and Telegraph Co, 7 Walnut St., Boston, Mass.	A June 26, 1891 M Dec. 16. 1891
TALTAVALL, THOS. R.	Editor, *Electrical Age*, World Building, New York City.	A Jan. 20, 1891 M Oct 27, 1891
TERRY, CHARLES A.	Lawyer, Westinghouse Electric Co, Pittsburgh, Pa.	A April 5, 1887 M May 17, 1887
THOMSON, PROF. ELIHU	(*Past President*). Electrician, Thomson Houston Electric Co., and Thomson Electric Welding Co., Lynn, Mass.	A April 15, 1884 M April 21, 1891
THOMPSON, EDWARD P.	Consulting Electrician and Patent Attorney in Electrical Cases, 5 Beekman St., New York.	A April 15, 1884 M Dec. 3, 1889
THURNAUER, ERNST	Manager, Thomson-Houston International Elec. Co., 7 Rue du Louvre, Paris, France.	A Oct. 14, 1887 M Dec. 6, 1887
TROWBRIDGE, PROF. W. P.	School of Mines, Columbia College. New York.	A April 15, 1884 M Oct. 21, 1884
TURNER, WILLIAM S.	Electrical Engineer, Woodbridge & Turner, 47 Times Building, New York.	A Dec. 7, 1886 M Oct. 2, 1888
VAIL, J. H.	Ass't Engineer in Chief, Edison General Electric Co., Edison Building, P. O. Box 3067, N. Y.	A June 8 1887 M Nov. 1, 1887
VAN DEPOELE, CHARLES J.	Electrician, Electrical Railway Department, Thomson-Houston Electric Co., Lynn, Mass.	A June 5, 1888 M Sept. 7, 1888
VANSIZE, WILLIAM B.	Solicitor of Patents, 44 Broad St., New York.	A April 15, 1884 M Oct. 21, 1884
WADDELL, MONTGOMERY	Engineer, The Waddell-Entz Electric Co., Bridgeport, Conn.	A Feb. 7, 1888 M May 1, 1888
WALKER, SYDNEY F.	Electrical Engineer, 195 Severn Road, Cardiff, England.	A June 2, 1885 M May 17, 1887
WALDO, DR. LEONARD	Electrical Engineer, The Aluminum Brass & Bronze Co., Bridgeport, Conn.	A June 5, 1888 M Dec. 4, 1888
WEAVER, W. D.	Hall Building, Troy, N. Y.	A May 17, 1887 M May 17, 1887
WEBB, HERBERT LAWS	26 West 34th Street, New York City.	A Oct. 21, 1890 M Dec. 16, 1890
WEEKS, EDWIN R.	General Manager, Kansas City Electric Light Co., National Bank of Kansas City Building. Kansas City, Mo.	A Sept. 6, 1887 M Nov. 1, 1887
WELLER, HARRY W.	Railroad Inspector, The Edison General Electric Co., Edison Building, Broad Street, New York City.	A Oct. 21, 1890 M Nov. 24, 1891
WESTON, EDWARD	(*Past President.*) Electrician, 645 High St., Newark, N. J.	A April 15, 1884 M Oct. 21, 1884

Name.	Address.	Date of Election.
WETZLER, JOSEPH	(*Vice-President.*) Editor *Electrical Engineer*, 150 Broadway, N. Y.	A April 15, 1884 M Dec. 9, 1884
WHARTON, CHAS. J.	82 Bond St., London England.	A Jan. 3, 1888 M May 1, 1888
WHEELER, SCHUYLER S., *Sc.D.*	President Crocker-Wheeler Electric Motor Co., 430-432 West Fourteenth St., Electrical Expert, Board of Electrical Control, 1266 Broadway, New York.	A June 2, 1885 M Sept. 1, 1885
WILKES, GILBERT	Edison General Electric Co., Edison Building, Box 3067, N. Y. City.	A Jan. 7, 1890 M Mar. 18, 1890
WILSON, FREMONT	Electrician, 293 Lenox (6) Ave., New York.	A Mar. 6, 1888 M June 5, 1888
WINCHESTER, A. E.	Designer of Steam Electrical Plants, Wilton, Conn.	A June 8, 1887 M Nov. 1, 1887
WOLCOTT, TOWNSEND	Consulting Electrician, 849 Greene Ave , Brooklyn, N. Y.	A Mar. 6, 1888 M Dec. 16, 1890
WOODBRIDGE, J. L.	Electrical Engineer, Woodbridge & Turner, 47 Times Building, New York,	A June 8, 1887 M Nov. 1, 1887
YOUNG, C. GRIFFITH	Superintendent and Electrician, Mount Morris Electric Co., Vandam and Greenwich Sts., New York City.	A Jan. 3, 1889 M April 21, 1891
ZETZSCHE, DR. CARL EDUARD,	Telegraph Engineer, Carlstrasse 8, II Dresden, N. Saxony.	A Nov. 1, 1887 M Jan. 3, 1888

Members, - - - 172.

ASSOCIATE MEMBERS.

ame.	Address.	Date of Election.
ABERNETHY, J. P.	Superintendent of Telegraph, 53 City Hall, Cleveland, O.	July 7, 1884
ABBOTT, ARTHUR V.	Mechanical Engineer, North Ave. Railway Co., Blackstone Building, Baltimore, Md.	Oct. 21, 1890
ALDEN, JAMES S.	Assistant Manager, with L. H. Alden, 486 River Drive, Passaic, N. J.	May 19, 1891
ALEXANDER, HARRY	Electrical Contractor, 126 Liberty and 340 W. 145th St., New York City.	April 21, 1891
ALEXANDER, P. H,	President, Alexander, Barney and Chapin, 20 Cortlandt St., New York City.	Dec. 16, 1890
ANDREWS, WM. S.	Supt. Construction Dept., Edison General Electric Co., Edison Building, Box 3067, New York.	Mar. 5, 1889
ANTHONY, WATSON G.	Assistant Electrician, The Okonite Co., 32½ Webster St., Newark, N. J.	Feb. 24, 1891
ATWOOD, GEORGE F.	Edison Laboratory, Orange, N. J.	Sept. 16, 1890
AYER, JAMES I	General Manager, Municipal Electric L. & P. Co., 322 Pine St., St. Louis, Mo.	May 19, 1891
AYRES, BROWN	Professor of Physics and Electrical Engineering, Tulane University, New Orleans, La.	Dec. 16, 1891

Name.	Address.	Date of Election.
BACKSTROM, CHAS. A.	Manufacturer of Electric Incandescent Lamps, (Present address unknown.)	Nov. 13, 1888
BAILLARD, E. V.	363 Manhattan Ave., New York.	Dec. 3, 1889
BARNARD, JOHN H.	General Manager, Asheville Street Railway Co., and Asheville Light and Power Co., Asheville, N. C.	June 26, 1891.
BARTON, ENOS M.	President Western Electric Co., 227 South Clinton St., Chicago, Ill.	July 12, 1887
BARRETT, JOHN A.	Electrical Engineer and Expert, 13 Park Row, New York City.	June 8, 1887
BATES, D. H.	Vice-President and Gen'l Manager, The Electrical Accumulator Co., 44 Broadway, New York.	April 15, 1884
BATES, F. C.	Electrical Engineer, with Thomson-Houston Electric Co., Lynn, Mass.	Jan. 20, 1891
BAUER, W. F.	Constructionist, The Electrical Accumulator Co., 62 Steuben St., East Orange, N. J.	April 15, 1890
BAYLES, ROBERT N.	Electrician, The "C & C" Electric Motor Co., 404 Greenwich St., New York.	Oct. 1, 1889
BEATTIE, JOHN, JR.	Manager and Superintendent, The Beattie Battery, Zinc and Electric Co., Fall River, Mass	Sept. 6, 1887
BEDELL, FREDERICK	Graduate Student, Cornell University, 118 Elm St., Montclair, N. J.	April 21, 1891
BENBOW, WILLIAM C.	Care Thomson-Houston Electric Co., Atlanta, Ga.	Nov. 12, 1889
BENJAMIN, PARK	Electrical Expert and Engineer, 32 Park Place, N. Y. City.	Dec. 16, 1891
BENNETT, J. C.	Electrician, United Edison Mfg. Co. Box 3067, 44 Broad St., New York.	Mar. 18, 1890
BERHGOLTZ, HERMAN	General Manager, The Wightman Elec. Mfg. Co., 4 Commonwealth Bldg., Scranton, Pa.	April 2, 1889
BERLINER, EMILE	Inventor, Columbia Road, between Fourteenth and Fifteenth Sts., Washington, D. C.	April 15, 1884
BISHOP, JAMES DRAPER	Electrical Expert, John A. Roeblings' Sons Co., 345 W. 34th St., New York City.	Dec. 16, 1891
BLACK, CHAS. N.	1113 Wilson Avenue, Cleveland, O.	Feb. 7, 1890
BLAKE, HENRY W.	The Street Railway Publishing Co., World Building, New York City.	Nov. 13, 1888
BLISS, DONALD M.	Electrician, The Canada Electric Co., Amherst, N. S.	Feb. 7, 1890
BLISS, WM. J. A.	820 Connecticut Ave., Washington, D. C.	Jan. 20, 1891
BLOOD, W. HENRY, JR.	Dealer in Electrical Supplies, Kansas City, Mo.	April 2, 1889
BOGART, A. LIVINGSTON	Electrical and Patent Expert, 22 Union Square, New York,	July 10, 1888
BOGUE, CHARLES J.	165 West Eighteenth St., New York,	Dec. 3, 1889

Name.	Address.	Date of Election.
BOTTOME, TURNER D.	Electrician and General Manager, White House Mills, Hoosick, N.Y.	April 21, 1891
BOTTOMLEY, HARRY	Electrical Engineer, 505 Exchange Bldg., Boston, Mass., 6 Farrar St., Lynn, Mass.	April 2. 1889
BOURNE, FRANK	Electrical Engineer, The Field Engineering Co., 143 Liberty St, New York City..	April 21, 1891
BOWMAN, FRED A.	Electrical Engineer, Royal Electric Co., Montreal, P. Q.	May 19, 1891
BRACKETT, PROF. CYRUS F.	Princeton, N. J.	April 15, 1889
BRADDELL, ALFRED E.	Electrical Inspector, Underwriters' Association. Middle Dep't, 308 Walnut St., Philadelphia, Pa.	Sept. 1, 1890
BRADLEY, FRED. W.	Electrical Staff World's Columbian Exposition, 15 Oakwood Avenue, Chicago, Ill.	May 20, 1890
BRADY, PAUL T.	State Agent, Thomson-Houston Electric Co., Syracuse, N. Y.	July 12, 1887
BRINER, C. J.	Vice-President. Central Electric Construction Co, 515 Walnut St., St. Louis, Mo.	April 2, 1889
BROPHY, WILLIAM	Chief Inspector, Electric Mutual Insurance Co., 85 Water St., Room 50, Boston.	Mar. 5, 1889
BROWN, ALEX. S.	Electrical Engineer, 136 Liberty St., New York City.	Jan. 7, 1890
BROWN, ALFRED S.	Electrical Engineer, Western Union Telegraph Co., 195 Broadway, New York City.	Mar. 18, 1890
BROWN, CHARLES A.	Attorney, Firm of Barton & Brown, Attorneys and Counsellors, 225 Dearborn St., Chicago, Ill.	July 12, 1887
BROWNING, HOWARD L.	Promoter and Contractor, 41 Dey St., New York.	Dec. 6, 1887
BUCKINGHAM, CHAS. L.	Patent Attorney, Western Union Telegraph Co., 195 Broadway, P. O Box 3393, New York.	April 15. 1884
BULKLEY, J. NORMAN	Thomson-Houston Electric Co., Lynn, Mass.	Oct. 21, 1890
BUNCE, THEODORE D., JR.	Battery Expert, The Consolidated Electric Storage Co., 477 No. Penna St., Indianapolis, Ind.	May 20, 1890
BURNS, ELMER Z.	Consulting Electrical Engineer, Room 55, 4th Floor. 120 Broadway, N. Y.	Feb. 7, 1890
BURLEIGH, CHAS. B.	Sup't. Isolated Dep't. Thomson-Houston Electric Co., 620 Atlantic Ave., Boston, Mass.	April 21, 1891
BURTON, GEO. D.	President, Electrical Forging Co., 194 Washington St., Boston, Mass.	April 21, 1891
BUYS, BERT.	Electrician, Rutherford B. S. & C. Elec. Co., Rutherford, N. J.	Feb. 7, 1890
CALDWELL, EDWARD	Assistant Editor, *Electrical World*, 167 Times Building, New York City.	Jan. 20, 1891
CALDWELL, FORDYCE S.	Proprietor Western Electric Construction Co, 503 Delaware St., Kansas City, Mo. and 151 Henry St., Brooklyn, N. Y.	Sept. 22, 1891

Name.	Address.	Date of Election.
CARTWRIGHT, FRED'K. G.	Electrical Engineer, and Agent Fort Wayne Electric Co., 35 New Montgomery St, San Francisco, Cal.	Sept. 22, 1891
CARTY, J. J.	Electrician, Metropolitan Telephone and Telegraph Co., 18 Cortlandt St., New York.	April 15, 1890
CASE, WILLARD E.	6 Fort St., Auburn, N. Y.	Feb. 7, 1888
CASPER, LOUIS	Wheatstone Repeater Chief, Western Union Telegraph Co., Cheyenne, Wyoming.	April 21, 1891
CHAMBERLAIN, F. H.	Electrician. Metropolitan R. R. Co., 2411 P St., N. W. Washington, D. C.	June 17, 1890
CHANDLER, CHARLES F.	Professor of Chemistry, Columbia College, and Prof. of Chemistry and Physics, College of Physicians and Surgeons, New York City.	Jan. 20, 1891
CHENEY, FREDERICK A.	Sec'y, Treas'r and General Manager, The Elmira Illuminating Co., Elmira, N. Y.	Oct. 1, 1889
CHENEY, W. C.	Electrical Engineer, Willamette Falls Electric Co., Portland, Oregon.	Sept. 22, 1891
CHERMONT, ANTONIO LEITE,	Manager Telephone Co. of Para, Para, U. S. of Brazil, Care Herbst Bros, Box 1377, New York.	Mar. 18, 1890
CHILD, FRANK W.	Contractor and Builder of Electric Railways, 115 Broadway, New York.	July 10, 1888
CHILDS, W. H.	Bookkeeper for The Estey Organ Co., Brattleboro, Vt.	Sept. 6, 1887
CHINNOCK, C. E.	Edison General Electric Co., Edison Building, P. O. Box 3067, New York.	April 15, 1884
CHUBBUCK, H. EUGENE	The New Omaha Thomson-Houston Electric Co., Omaha, Neb.	Dec. 4, 1888
CHURCHILL, ARTHUR.	Engineering Dept., Schenectady Works, Edison General Elec Co., 25 Front St., Schenectady, N. Y.	April 15, 1890
CLEMENT, LEWIS M.	1013 Central Ave., Oakland, Cal.	April 21, 1891
CLEVELAND, WM. B.	Electrical Engineer, 309 Perry-Payne Building, Cleveland, O.	April 15, 1884
COBB, JOHN S.	Electrician, 136 Liberty St., New York.	June 17, 1890
COFFIN, CHAS. A.	Vice-President and Treasurer, Thomson-Houston Electric Co., 620 Atlantic Ave., Boston, Mass.	Dec. 6, 1887
COGSWELL, A. R.	Electrician and Superintendent, Halifax Illuminating and Motor Co. Ltd., 34 Bishop St, Halifax, N. S.	April 21, 1891
COLGATE, GEO. L.	Electrical Engineer, Bedford Electric Co., Bedford City, Va.	June 17, 1890
COLLEY, BENJAMIN W.	First Ass't Superintendent, The Commercial Cable Co, Hazel Hill, N. S.	Oct. 21, 1890
COLVILLE, FRANK C.	Electrician and Inventor, 1503 Seventh Ave, Oakland, Cal.	May 19, 1891

Name.	Address.	Date of Election.
COMPTON, ALFRED G.	(*Manager.*) Professor of Physics, College of the City of New York, 17 Lexington Ave., New York.	Nov. 1, 1887
CORNELL, CHAS. L.	Electrical Engineer, Cornell Engineering Co., 45 Broadway, N. Y.	Feb. 7, 1890
CRAM, HENRY B.	Treasurer, Bernstein Electric Co., 620 Atlantic Ave., Boston, Mass.	June 26, 1891
CRANE, W. F. D.	Engineer, Engineering Equipment Co., 143 Liberty St, New York City., and 24 Halstead Pl., East Orange, N. J.	Feb. 7, 1888
CROSBY, OSCAR T.	General Manager Railway Dep'tment, Thomson-Houston Electric Co., 620 Atlantic Ave., Boston, Mass.	Mar. 18, 1890
CUNTZ, JOHANNES H.	Assistant to Pres't Henry Morton, Stevens Institute of Technology, 137 Hudson St, Hoboken, N. J.	Mar. 5, 1889
CURTIS, CHAS. G.	63 Broadway, and 441 Park Ave., New York.	April 15, 1884
CUSHING, F. W.	Secretary, Central Electric Co., 118 Franklin St., Chicago, Ill.	Nov. 24, 1891
CUSHMAN, HOLBROOK	Instructor in Physics, Columbia College, 337 West Twenty-second St., New York.	June 5, 1888
DAME, FRANK L.	Engineering Dep't., The Northwest Thomson-Houston Electric Co., Front and A Sts., Portland, Or.	June 26, 1891
DANA R. K.	Agent Washburn and Moen M'f'g Co., 16 Cliff St, New York.	April 15, 1884
DANIELL, FRANCIS G.	Care of Barry & McTighe, 52 and 54 William St., New York City.	Nov. 12, 1889
DAVENPORT, GEORGE W.	General Manager, Thomson-Houston International Electric Co., 620 Atlantic Ave., Boston, Mass.	June 4, 1889
DAVIDSON, EDW. C.	Patent Lawyer, Room 179, Times Bldg., New York.	Feb. 7, 1890
DAVIS, CHAS. M.	Superintendent, Light, Heat & Power Co., Pueblo, Col.	July 12, 1887
DAVIS, DELAMORE L.	Superintendent, Salem Electric Light & Power Co., Salem, O.	April 2, 1889
DAVIS, JOSEPH P.	Consulting Engineer, Care of Phœnix Construction Co., 115 W. 38th St., New York City.	April 15, 1884
DAVIS, MINOR M.	Ass't Electrician, Postal Telegraph Cable Co, 5 Dey St, New York.	April 6, 1886
DE KHOTINSKY, CAPT. ACHILLES,	Electrician, Germania Electric Co., 505 Exchange Bldg., Boston, Mass.	Oct. 27, 1891
DENTON, JAMES E.	Professor of Experimental Mechanics, Stevens Institute of Technology, Hoboken, N. J..	July 12, 1887
DICKERMAN, GEORGE W.	1007 Asylum Ave., Hartford, Conn.	Nov. 18, 1890
DICKERSON, E. N.	Attorney-at-Law, 15 Wall St., N. Y.	April 15, 1884
DION, ALFRED A.	Manager, Ahearn & Soper, Ottawa, Ont.	Jan. 7, 1890

Name.	Address.	Date of Election.
DOANE, S. EVERETT	Thomson-Houston Electric Co., Swampscott, Mass.	Aug. 6, 1889
DOBBIE, ROBERT S.	Edison General Elec. Co., Broad St., N. Y. City. Box 3067.	Feb. 5, 1889
DOREMUS, CHARLES A.	*M. D. Ph. D.* Chemist and Physicist, Bellevue Hospital Medical College, College of the City of New York and American Veterinary College, 92 Lexington Ave., New York.	July 7, 1884
DRESSLER, CHARLES E.	Maker of Scientific and Electrical Apparatus, College of the City of New York, 17 Lexington Ave., New York City.	Dec. 16, 1890
DUNDERDALE, CLEAVELAND F.	Agent Westinghouse Electric Co., 146 Adams St., Chicago, Ill.	June 17, 1890
DUNN, GANO S.	Electrical Engineer, of the Crocker-Wheeler Electric Co , 430 West 14th St., Residence, 223 Central Park, West, New York City.	April 21, 1891
DUNSTON, ROBERT EDWARD,	President, The Connecticut Motor Co.. Plantsville, Ct.	Oct. 27, 1891
DURANT, GEO. F.	Vice-Pres't of Bell Telephone Co. of Mo., 322 Pine St., St. Louis, Mo.	April 15, 1884
EKSTROM, AXEL	Electrician, Thomson-Houston Electric Co., Lynn, Mass.	June 17, 1890
ELEY, HARRIS H.	Electrical Workshop Sup't. W. C. & S. W. Telephone Co., 88 Colston St., Bristol, Eng.	Jan. 7, 1890
ELMER, WILLIAM, JR.	Under-graduate in Electrical Engineering, Princeton University, Princeton, N. J.	Mar. 18, 1890
EMERY, CHARLES EDWARD	Consulting Engineer, Bennet Building, cor. Fulton and Nassau Sts., New York.	June 26, 1891
EMMET, HERMAN L. R.	Publisher and Printer, 36 Cortlandt St., New York.	April 15, 1884
ENTZ, JUSTUS B.	Electrical Engineer, The Waddell-Entz Co., Bridgeport, Ct.	Jan. 7, 1890
ESSICK, SAMUEL V.	Electrician, The Essick Printing Tel. Co., 171 Broadway, New York City, and Yonkers, N. Y.	May 19, 1891
EVEREST, AUGUSTINE R.	Electrical Expert, Thomson Electric Welding Co., 89 State St., Boston, Mass.	May 19, 1891
FAY, THOMAS J.	General Manager, Maine Electric Improvement Co., 136 Liberty St., New York.	June 26, 1891
FIELDING, FRANK E.	Chemist and Assayer, Virginia City, Nev.	Sept. 6, 1887
FISCHER, GUSTAVE J.	Engineer for Tramway Construction, Public Works Department, Sydney, N. S. W.	Jan. 20. 1891
FISH, WALTER C.	Manager, Bernstein Electric Co., 620 Atlantic Ave., Boston, Mass.	June 26, 1891
FISKE, HENRY G.	Electrician, Croton Magnetic Iron Mines. 45 East Twenty-second Street, New York.	Nov. 12, 1889

Name.	Address.	Date of Election
FISKE, J. P. B.	Electrical Engineer, Thomson-Houston Electric Co., Lynn, Mass.	June 17, 1890
FITCH, GRAHAM D.	First Lieut. Corps of Engineers, U. S A. Portland, Or.	Oct. 21, 1890
FLACK, J. DAY	Ass't Electrician in charge of Tests, Edison Lamp Works, Harrison, N. J.	Dec. 6, 1887
FLOOD, J. F.	Supt. Steubenville Street Railway Co., Steubenville, O.	Mar. 18, 1890
FOOTE, ALLEN R.	Special Agent, Electrical Industries, U. S. Census, P. O. Box 432, Washington, D. C.	April 21, 1891
FOOTE, CHARLES W.	Agent, Railway Dept., Thomson-Houston Elec. Co., Society for Savings Bldg., Cleveland, O.	Sept. 22, 1891
FOOTE, THOS. H.	104 Columbia Heights, Brooklyn, N Y.	April 21, 1891
FORBES, FRANCIS	Lawyer, 137 Broadway. New York City.	Sept. 16, 1890
FOX, JOHN M.	Electrician, Room 201, 18 Broadway, New York City.	Mar. 18, 1890
FRANCISCO, M. J.	President and General Manager, Rutland Electric Light Co., Rutland, Vt.	June 17, 1890
FREEDMAN, WILLIAM H.	Honorary Fellow in Electrical Engineering, Columbia College, 120 West 125th Street, New York.	Mar. 18, 1890
FRENCH, E. L.	Draughtsman, Mather Electric Co., Manchester, Conn.	Dec. 16, 1890
FULLER, LEVI K.	Vice-President, Estey Organ Co., Brattleboro, Vt.	Mar. 5, 1889
GAINES, J. D.	Second Ass't. Superintendent, The Commercial Cable Co., Hazel Hill, N. S.	Oct. 21, 1890
GANS, JOHN L.	Superintendent and Electrician, The Electric Company, Connellsville, Penn.	June 26, 1891
GILES, WALTER A	Engineer and Contractor, 416 Lewis Block, Pittsburgh, Pa.	Nov. 1, 1887
GILLILAND, E. T.	179 West End Ave., New York.	April 15, 1884
GOLDMARK, CHAS. J.	473 Park Ave., New York.	June 5, 1888
GORTON, CHARLES	Civil Engineer, Belmont, N. Y.	Nov. 12, 1889
GORDON, REGINALD	Assistant in Physics, Columbia College, New York City, Residence, 76 Park Ave.	Feb. 24, 1891
GRAY, W. N.	Electrical Engineer, 12 Chamber of Commerce, Cincinnati, O.	Oct. 1, 1889
GREENBERG, ADOLPH G.	Salesman, Fort Wayne Electric Co., 115 Broadway, New York City, 216½ 17th Street, Brooklyn, N. Y.	Mar. 17, 1891
GROWER, GEORGE G.	Electrician and Chemist, Ansonia Brass and Copper Co., Ansonia, Conn.	Mar. 18, 1890

Name.	Address.	Date of Election.
GRIFFIN, CAPT. EUGENE	Thomson-Houston Elec. Co., 295 Commonwealth Ave., Boston, Mass.	Feb. 7, 1890
GUTMANN, LUDWIG	Electrical Engineer, P. O. Box 560, Pittsburgh, Pa.,	Sept. 14, 1888
HADLEY, WARREN B.	Supt. Wiring Dept., Edison Electric Illuminating Co., 431 Fifth Ave., New York.	June 26, 1891
HALL, EDWIN H.	Assistant Professor of Physics, Harvard College, Gorham St., Cambridge, Mass.	Sept. 3, 1889
HALL, JOHN L.	Manager, Western Union Telegraph Co., 300 Market St., Wilmington, Del.	Sept. 22, 1891
HALL, WILLIAM P.	President, The Hall Signal Co., 50 Broadway, New York City.	Sept. 16. 1890
HALSEY, WILLIAM B.	Inspector, Postal Telegraph-Cable Co., 94 Leonard St., New York.	Mar. 18, 1890
HAMILTON, WILLIAM H.	Dealer in Electrical Supplies, 100 State St., Albany, N. Y.	Sept. 22, 1891
HANCOCK, L. M.	Supt. of Construction, Western Electric Co., 22 Thames St., New York City.	May 19, 1891
HANDLEY, ARTHUR	Electrical Engineer. Electrical Engineering Co. of Ireland, Limited, 61 Dawson St., Dublin, Ireland.	Dec. 16, 1890
HARDING, H. McL.	Railway Dep't, Westinghouse Electric Co , Pittsburgh, Pa.	May 24, 1887
HARRINGTON, WALTER E.	Wheeling Electric Railway, Wheeling, W. Va.	Mar. 17, 1891
HART, FRANCIS R.	Supt. of Construction, Mass. Electrical Engineering Co., 4 P. O. Square, Boston, Mass.	April 21, 1891
HASKINS, CARYL D.	Electrician, Thomson-Houston Electric Co., Lynn, Mass.	Mar. 18, 1890
HASSON, W. F. C.	Assistant Engineer. U. S. N. Instructor Johns Hopkins University, Baltimore, Md.	Mar. 18, 1890
HATZEL, J. C.	Electrical Engineer and Contractor, 29 West Twenty-sixth St., New York.	Sept. 3, 1889
HEALY, LOUIS W.	The Wightman Electric Co., 1205 Marion St , Scran on, Pa.	June 26, 1891
HEILMAN, DONALD B.	Jonestown, Pa.	Mar. 18, 1890
HEINRICH, RICHARD O.	Demonstrator in Electrical Engineering and Physics, Lehigh University, South Bethlehem, Pa.	Oct. 1, 1889
HENDERSON, ALBERT H.	315 Drexel Bldg., Philadelphia. Pa,	July 12, 1887
HENRY, JOHN C.	Electrical Engineer. 114 E. 54th St., New York City.	April 21, 1891
HENSHAW, FREDERICK V.	Assistant Electrician, The "C.& C." Electric Motor Co., 402 Greenwich St , New York.	Feb. 5, 1889
HERING, HERMANN S.	Johns Hopkins University, Baltimore, Md.	April 21, 1891

Name.	Address.	Date of Election.
HERRICK, CHARLES H.	Manager and Electrical Engineer, Wright Electrical Engineering Co. Co., 196 Summer St., Boston, Mass.	April 21, 1891
HEWITT, CHARLES	The Edison General Electric Co., Edison Building, Broad St., Box 3067, New York City.	Sept. 16. 1890
HEWLETT, EDWARD M.	Electrical Engineer, Isolated Dept. Thomson-Houston Electric Co 65 Columbia Heights, Brooklyn, N. Y.	May 19, 1891
HIBBARD, ANGUS S	General Superintendent, American Telephone and Telegraph Co., 18 Cortlandt St., N. Y. City.	Nov. 24, 1891
HOCHHAUSEN, WILLIAM	Electrician, The Excelsior Electric Co , 196 Willoughby St., Brooklyn, N. Y.	April 15, 1884
HOFFMAN, SAMUEL V.	Post-graduate Student, Johns Hopkins University, Baltimore, Md.	Jan. 20, 1891
HOLCOMB, EUGENE R.	Engineering Dept., Edison Gen'l Elec. Co., Portland, Ore.	June 17, 1890
HOLMES, FRANKLIN S.	Electrical Engineer, with C. H. Davis, 120 Broadway, N. Y , 445a Macon St., Brooklyn, N. Y.	April 21, 1891
HOLT, MARMADUKE BURRELL,	Aspen Mining and Smelting Co., Aspen, Col.	April 15, 1890
HOPKINS, J. HERBERT	403 North 8th St., St. Louis, Mo.	April 2, 1889
HOSFORD, HENRY H.	Electrical Engineer, 1009 Society for Savings Bldg., Cleveland, O.	Sept. 22, 1891
HOWSON, HUBERT	Patent Lawyer, 38 Park Row, New York.	June 8, 1887
HUBRECHT, DR. H. F. R.	Director. Nederlandsche Bell Telephone Co., Amsterdam, Holland.	Oct. 4, 1887
HUFF, S. W.	Manager. Electrical Department of Factory, Baxter Electric Motor Co., 2100 Druid Hill Ave., Baltimore, Md.	Nov. 24, 1891
HUMPHREYS, C. J. R.	Manager, Lawrence Gas Co., Lawrence, Mass.	Sept. 6, 1887
IDELL, FRANK E.	Mechanical Engineer 41 Dey St., New York.	July 12, 1887
IHLDER, JOHN D.	Electrical Engineer, with Osterheld & Eickemeyer, Manuf'rs of Dynamos and Motors, Yonkers, N. Y.	Oct. 2, 1888
INSULL, SAMUEL	Electrical Manufacturer, Box 3067, New York.	Dec. 7, 1886
IVES, EDWARD B.	Lieutenant U. S. A., Electrical Engineer, 139th St. and Grand Boulevard, New York.	April 2, 1889
IZARD, E. M.	Electrical Engineer, The Electrical Engineering Co , 320 Dearborn St., Chicago, Ill.	Mar. 5, 1889
JACKSON, C. H.	President, United Electric Light and Power Co., 59 Liberty St., New York.	May 24, 1878

Name.	Address.	Date of Election.
JOHNSTON, A. LANGSTAFF	Civil and Consulting Engineer in the the General Construction of Electric Railways, 1105 E. Main St., Richmond. Va.	April 21, 1891
JOHNSTON, W. J.	President, The W. J. Johnston Co., Ltd., Times Building, P. O. Box 3332, N. Y.	April 15, 1884
JONES, F. R.	University of Tennessee, Knoxville, Tenn.	May 20, 1890
JUDSON, WM. PIERSON,	U. S. Civil Engineer, Oswego, N. Y.	June 8, 1887
KEEN, WILLIAM M. B. *M.D.*	85 Varick Street, New York City.	Sept. 16, 1890
KELLOGG, JAMES W.	Assistant to District Engineer, Edison General Electric Co., 140 E. 27th St , New York.	June 26, 1891
KENNELLY, A. E.	Electrician, Edison Laboratory, Orange, N. J.	May 1, 1888
KIMBALL, A. S.	Professor of Physics, Worcester Polytechnic Institute, Worcester, Mass.	Sept. 3, 1889
KINNEY, H. A.	Representative Brush Electric Co., N. Y. Life Ins. Building. Omaha, Neb.	Mar. 18, 1890
KREIDLER, W. A.	Editor and Publisher, *Western Electrician*, 6 Lakeside Building, Chicago, Ill.	Oct. 4, 1887
LAIN, DAVID E., *B. S.*	Electrical Engineer, Yonkers, N. Y.	Nov. 13, 1888
LAND, FRANK	606 Salina St., Syracuse, N. Y., Hotel Normandie, N. Y. City.	Sept. 22, 1891
LAUDY, LOUIS H.	Assistant in Applied Chemistry, School of Mines, Columbia College, New York City, Residence, 239 West 52d St.	Feb. 24, 1891
LAW, MYRON D.	Manager Arc Light Dep't., Edison Machine Works, Schenectady, N. Y.	Feb. 7, 1888
LEDOUX, A. R.	Chemical Expert, 10 Cedar St., New York.	Dec. 7, 1886
LEE, HUNTINGTON	Pittsfield, Mass.	Dec. 3, 1889
LEE, JOHN C.	Chemist and Electrician, American Bell Telephone Co., Mountfort St., Longwood, Brookline, Mass.	Mar. 18, 1890
LEMP, HERMANN, JR.	Electrician, Thomson Electric Welding Co , Lynn, Mass.	April 2, 1889
LEVY, ARTHUR B.	Assistant Engineer, Arc Light Dep't, Edison General Electric Co., 235 E. 50th St., N. Y. C.	Jan. 20, 1891
LEWIS, HENRY FREDERICK WILLIAM,	General Manager and Secretary of the Western Counties South Wales Telephone Co., England, 8 Meridian Road, Redland, Bristol, Eng.	Mar. 5, 1889
LIEBIG, GUSTAV A., JR.	Elec'l Testing Bureau, Johns Hopkins University, Baltimore, Md.	Mar. 6, 1888
LLOYD, ROBERT MCA.	Electrician of the Marion, N. J. Factory, United Electric Traction Traction Co., 2 W. 36th St., New York City.	Oct. 21, 1890

Name.	Address.	Date of Election.
LOGAN, CHARLES H.	General Manager, Standard Electric Co., Detroit, Mich.	Nov. 18, 1890
LOOMIS, OSBORN P.	Electrician, Eureka Electric Co., 530 Nostrand Ave., Brooklyn, N.Y.	Sept. 16, 1890
LOVEJOY, J. R.	Electrical Engineer, Thomson-Houston Electric Co., 620 Atlantic Ave., Boston, Mass.	April 21, 1891
LOWREY, GROSVENOR P.	Lawyer, 15 Broad St., Residence 121 Madison Ave., New York.	Nov. 1, 1887
LOZIER, ROBERT T. E.	Electrical Expert, The Edison General Electric Co., Broad Street, Box 3067, New York.	May 20, 1890
LUFKIN, HARVEY L.	General Agent, "C & C" Electric Motor Co. 404 Greenwich St., New York.	June 17, 1890
LUNDELL, ROBERT	Electrical Engineer, Interior Conduit and Insulation Co., 44 Broad St., New York. Residence 1309 Bedford Ave., Brooklyn, N. Y.	Feb. 7, 1890
LUQUER, THATCHER T. P.	Asst. in Surveying and Practical Mining, and Student in Electrical Engineering, Columbia College, N. Y., and Bedford, N. Y.	June 26, 1891
MACMULLAN, ROBERT HEATH,	Treasurer and General Manager, Brush Electric Lighting Co., Lafayette, Ind.	Sept. 22, 1891
MAC QUESTEN, W. D.	Electrical Engineer and Contractor, 143 Liberty St., New York.	April 15, 1890
MADDEN, O. E.	136 Liberty St., New York.	April 15, 1884
MAGEE, LOUIS J.	Electrical Engineer, in charge of European Office of Thomson-Houston International Electric Co., Michaelisbrücke 1, Hamburg, Gy.	April 2, 1889
MALCOLM, PHILIP S.	Western Agent, Gibson Electric Co., Portland, Oregon.	Mar. 18, 1890
MANSFIELD, GEO. W.	Electrical Engineer, with Thomson-Houston Electric Co., 620 Atlantic Ave., Boston, Mass.	June 2, 1885
MARKS, LOUIS B.	Washington Carbon Co., Washington, Pa.	May 20, 1890
MARPLE, LUCIUS E.	Electrician, United Electric Railway, Nashville, Tenn, and Wollaston, Mass.	Sept. 22, 1891
MARTIN, F.	Inspector of Isolated Plants, Edison Electric Illuminating Co., of N. Y. "Nevada" 70th St., and Boulevard, New York City.	Oct. 21, 1890
MARTIN, J.	Electrician, 16 Oak St., Newark, N. J.	Oct. 21, 1890
MARTIN, T. COMMERFORD	(*Past President.*) Editor, *The Electrical Engineer*, 150 Broadway, New York.	April 15, 1884
MASON, AUGUSTÉ F.	General Manager, Simplex Electric Co, 620 Atlantic Avenue, Boston, Mass.	June 17, 1890

Name.	Address.	Date of Election.
MASON, JAMES H.	Electrician and Supt., Mason Battery and Electrical Co., 120 Park Ave., Brooklyn, N. Y.	May 19, 1891
MAYER, GEORGE	Mechanical Draughtsman, Edison General Electric Co., 68 Broad St., New York City.	Dec. 16, 1890
MCKIBBIN, GEORGE N.	Chemist and Electrician, Reed & McKibbin, Consulting Electrical Engineers and Contractors, 2 Wall St., New York.	June 8, 1887
MCKINSTRY, J. P.	185 Seneca St., Cleveland, O.	April 15, 1884
MERCER, ANDREW G.	Treasurer and Electrician, Waterloo Electric Co., Waterloo, N. Y.	Sept. 3, 1889
MERRITT, ERNEST	Instructor in Physics, Cornell University, Ithaca, N. Y.	Sept. 16, 1890
MIDDLETON, EDWARD T.	Instructor in Electricity and Physics. Rutgers College, New Brunswick, N. J.	Oct. 27. 1891
MILLER, JOSEPH A.	Civil and Consulting Engineer, Providence. R. I.	Dec. 9, 1884
MILLER, WM. C.	General Manager, Watervliet Turnpike & R. R. Co., 3 South Hawk Street, Albany, N. Y.	Oct. 21, 1890
MINER, W. M.	Electrician and Inventor, 89 East Second St., Plainfield, N. J.	July 12, 1887
MITCHELL, JOHN MURRAY	Lawyer, Box 3712, 45 Wall St., New York.	June 2, 1885
MITCHELL, SIDNEY Z.	Manager, Oregon, Washington and Idaho Agency, Edison General Electric Co., Fleischner Building, Portland, Or.	Nov. 12, 1889
MIX, EDGAR W.	Electrician, with Thomson-Houston Electric Co., Lynn, Mass.	Sept. 3, 1889
MONELL, JOSEPH T.	With F. B. Crocker. 236 W. 22d Street, New York City.	Oct. 27, 1891
MOORE. JOHN J.	425 E. Twenty-fourth St, New York.	Nov. 12, 1889
MORDEY, WM. MORRIS	Electrician, Brush Electrical Engineering Co, 34 Montserrat Road, Putney, London, Eng.	Sept. 22, 1891
MORRISON, J. FRANK	15 South St., Baltimore, Md.	April 15, 1884
MORSS, EVERETT.	Electrician, Simplex Electric Co., 297 Beacon St., Boston, Mass.	Sept. 22, 1891
MORTON, HENRY, *Ph. D*	President of Stevens Institute of Technology, Hoboken, N. J.	May 24, 1887
MOSES, DR. OTTO A.	Electrician, 131 East Seventy-third St., New York.	May 17, 1887
MOSS, GEO. W.	Station Manager, C. & S. A. Tel. Co., Tehuantepec, Mexico.	Jan. 7, 1890
MOSSCROP, WM. A.	Sup't of Construction, Woodbridge & Turner, Room 44, The Brevoort, Brooklyn, N. Y.	May 7, 1889
MYERS, GEO. FRANCIS	Electrical Engineer, Westinghouse Bldg. Penna Ave., and 9th St., Pittsburgh, Pa.	June 17, 1890
NESMITH, S. D.	Manager, Cleveland Office, Thomson-Houston Electric Co., 48 Arcade, Cleveland, O.	Sept. 16, 1890

Name.	Address.	Date of Election.
NEWELL, ARTHUR J,	Electrical Engineer, R. T. Oakes & Co., 184 Beech St., Holyoke, Mass.	Mar. 18, 1890
NICHOLS, ARTHUR E.	Student in Electrical Engineering, Columbia College, 16 E. 35th St., New York City.	Mar 18, 1890
NUNN, RICHARD J., *M. D.*	Physician, 119½ York St., Savannah Ga.	July 12, 1887
OCKERSHAUSEN, H. A.	Electrical Engineer, 65 Madison Ave., Jersey City, N. J.	Sept. 6, 1887
O'DEA, M.	Electrician, University of Notre Dame, Notre Dame, Ind.	June 8, 1887
OTTEN, DR. JAN D.	Engineer, Thomson-Houston International Electric Co., Michaelsbrucke 1, Hamburg, Germany.	Nov. 18, 1890
OWENS, R. B.	Professor of Electrical Engineering, University of Nebraska, Lincoln, Neb.	June 17, 1890
PALMER, G W., JR	Address Unknown.	April 15, 1890
PARCELLE, ALBERT L.	Electrician and Inventor, 157 Washington St., Boston, Mass.	Dec. 16, 1891
PARSELL, HENRY V., JR.	31 East Twenty-first St., New York.	Nov. 12, 1889
PAUL, CHAS. M.	Electrician, 172 Remsen St., Brooklyn, N. Y.	May 7, 1889
PECK, EDWARD F.	General Sup't Citizens Electric Illuminating Co, Cor. Rockwell Place and DeKalb Ave., Brooklyn, N. Y.	May 20, 1890
PECK, SAMUEL C.	Electrician, Thomson-Houston Electric Co., 620 Atlantic Ave, Boston, Mass.	Sept 6, 1887
PEIRCE, WM. H.	Consulting Engineer, Keyser B'ld'g, German and Calvert Sts., Baltimore, Md.	Sept. 7, 1888
PERKINS, FRANK C,	209 Central Ave., Dunkirk, N. Y.	Oct. 21, 1890
PHILLIPS, EUGENE F.	M'f'r Insulated Electric Wire, Providence, R. I.	July 13, 1889
PIKE, CLAYTON W.	Instructor in Electrical Engineering University of Pa., Philadelphia, Pa.	Dec. 16, 1891
POOLE, CECIL P.	Contracting Electrical Engineer, 206–8 Eighth St. Lynchburg, Va.	Jan. 3, 1888
POOR, CHAS. LANE	Johns Hopkins University, Baltimore, Md.	Dec. 16, 1890
PUPIN, DR. MICHAEL I.	Instructor in Mathematical Physics, Columbia College, 68 W. 72d St., New York City.	Mar. 18, 1890
POPE, RALPH W.	Secretary to the American Institute of Electrical Engineers, 12 W. 31st St., Editor *Electric Power*, 136 Liberty St., New York. Residence, 570 Cherry St., Elizabeth, N. J.	June 2, 1885
POWELL, WILLIAM H.	New Britain, Conn.	June 17, 1890
PUTNAM, H. ST. CLAIR	Treasurer and Electrician, Thomson-Houston Carbon Co., Fremont, O.	Sept. 16, 1890
RAE, FRANK B.	Electrical Engineer, Detroit Electrical Works, Detroit, Mich.	April 15, 1884

Name.	Address.	Date of Election.
RANDALL, JOHN E.	Incandescent Lamp Dep't, Thomson-Houston Electric Co., Lynn, Mass.	May 7, 1889
REED, CHAS. J.	Electrician, 224 High St., Orange, N. J.	Mar. 5, 1889
REED, HENRY A.	Secretary and Manager, Bishop Gutta-Percha Co., 422 East Twenty-fifth St., New York.	June 4, 1889
REID, THORBURN	Edison General Electric Co., Schenectady, N. Y.	Oct. 21, 1890
REILLY, JOHN C.	General Sup't, N.Y. & N. J. Tel. Co., 16 Smith St., Brooklyn, N. Y.	April 15, 1884
REINMANN, A. L.	Electrician, 510 W. 23d St., New York City.	June 8, 1887
REIST, H. G.	Electrical Engineer, Thomson-Houston Electric Co., 113 Franklin St., Lynn, Mass.	June 17, 1890
RIKER, ANDREW L.	Electrical Engineer, 15 East Fifty-fifth St., New York.	Nov. 1, 1887
ROBB, WM. LISPENARD	Professor of Physics, Trinity College, Hartford, Conn.	Dec. 16, 1891
ROBINSON, ALMON,	Draughtsman, Expert in Methods of Gearing, P. O. Box, 943, Lewiston, Me.	Sept. 6, 1887
RODMAN, SAMUEL, JR.	Lieut 1st Artillery, U. S. A., Fort Adams, Newport R, I.	Sept. 16, 1890
KOEBLING, FERDINAND W.	Manufacturer of Electrical Wires and Cables, Trenton, N. J.	June 8, 1887
ROESSLER, S. W.	Captain, U. S. A., U. S. Engineer Office, Memphis, Tenn.	Dec. 3, 1889
ROGERS, EDWARD H.	Patent Lawyer, firm of Pope and Rogers, 15 Wall St., New York. City.	Sept. 22, 1891
ROGERS, WINFIELD S.	Mechanical Engineer, Estate of F. W. Richardson, Troy, N. Y.	Sept. 3, 1889
ROSEBRUGH, THOMAS REEVE	Lecturer in Electrical Engineering, School of Practical Science, Toronto, Ont.	June 26, 1891
ROSENBAUM, WM. A.	Electrical Patent Solicitor, Care of *The Electrical World*, Times Building, New Yo k.	Jan. 3, 1889
ROSENBERG, E. M.	Supt. Manhattan E. L. Co.. 80th St., and Ave. B Residence, 784 Lexington Ave., N. Y. City.	Oct. 21, 1890
ROYCE, FRED W.	Electrician and Patent Solicitor, 1408 Pennsylvania Ave., Washington D C.	April 15, 1884
RUTHERFORD, W. M.	Toronto Construction and Electrical Supply Co., 63 Front Street, W. Toronto, Can	Sept. 22, 1891
RYAN, HARRIS J.	Professor of Electrical Engineering, Cornell University, Ithaca, N. Y.	Oct. 4, 1887
SANBORN, FRANCIS N.	Manhattan Electric Lighting Co., 13 Spencer Place, Brooklyn, N. Y.	Nov. 24, 1891
SARGENT, W. D.	General Manager, N. Y. & N. J. Tel. Co., 16 Smith St., Brooklyn, N. Y.	April 15, 1884
SAWYER, FREDERICK J	381 Boylston St., Boston, Mass.	June 8, 1887

Name.	Address	of Election.
SAXELBY, FREDERICK	Electrical Engineer, 416 Jersey St., Harrison, N J.	June 5, 1888
SCHLOSSER, FRED. G.	Superintendent of Electric Dept., Laclede Gas Light Co., 1801 Wash St., St. Louis, Mo.	Sept. 22, 1891
SCHMID, ALBERT	Superintendent, Westinghouse Electric & Mfg. Co., Pittsburgh, Pa.	Oct 21 1890
SCHMIDT, FREDERICK	Managing Director, The Schmidt-Douglas Electric Co., L'td., 31 Blenheim Rd., Bradford, England.	Jan. 3, 1888
SCHMIDT, GIACOMO	Ass't General Manager, Richmond Light, Heat and Power Co., New Brighton, N. Y.	April 21, 1891
SEARING, LEWIS	Thomas, Shepard & Searing, 69 Arapahoe Building, Denver, Colorado.	April 3, 1888
SEELY, J. A.	General Manager, Complete Construction Co., 10 Cortlandt St., New York.	April 15, 1884
SERRELL, LEMUEL WM.	Mechanical and Electrical Engineer, 10 Wall St., New York.	Nov. 1, 1887
SHAW, GEORGE B.	General Manager, National Electric Mfg. Co., Eau Claire, Wis.	April 15, 1890
SHEBLE, FRANKLIN	Electrical Engineer, Thomson-Houston Electric Co., Lynn, Mass.	Oct. 21, 1890
SHEEHY, ROBERT J.	Electrical Engineer, Equitable Mfg. and Electric Co., 24 West St. and 101 W. 76th St., New York City.	April 21, 1891
SHEEHAN, WILLIAM M.	Manager and Electrician, Newburgh Electric Light & Power Co., 65 Montgomery St., Newburgh, N.Y.	Sept. 3, 1889
SHEPARDSON, GEORGE D.	Dep't. of Electrical Engineering, University of Minnesota, Minneapolis, Minn.	April 21, 1891
SINCLAIR, H. A.	Electrical Engineer. The Tucker Electric Co., 950 Bedford Ave., Brooklyn, N. Y.	June 17, 1890
SISE, CHARLES F.	Vice-President and Managing Director, Bell Telephone Co., of Canada, and Canadian Telephone Co , Ltd., Montreal, Canada.	June 8, 1887
SMITH, FREDERICK H.	Civil Engineer. 227 East German St., Baltimore, Md.	Nov. 12, 1889
SMITH, GERRITT	Circuit Electrician, 195 Broadway, P. O. Box 3393, New York.	April 15, 1884
SMITH, HAROLD BABBITT.	Adj. Prof. in charge of Electrical Engr'g Dept., Arkansas State University, Fayetteville, Ark. Barre, Mass (till Mar 2.)	Nov. 24, 1891
SMITH, J. ELLIOT	Superintendent Fire Alarm Telegraph, 157 East Sixty-seventh St , New York.	April 15, 1880
SMITH, OBERLIN	President and Mechanical Engineer, Ferracute Machine Co., Lochwold, Bridgeton, N. J.	May 19, 1891
SOUZA, CARLOS MONTEIRO	e Rio de Janeiro, Brazil.	Sept. 6, 1887

Name.	Address.	Date of Election.
SPAULDING, HOLLON C.	Sec'y, Thomson-Van Depoele Electric Mining Co., 620 Atlantic Ave., Boston, Mass.	April 21, 1891
SPICER, CHAS. W. W.	122 W. 21st Street, New York City.	Nov. 12, 1889
SPIKE, CLARENCE J.	Halifax, N. S	Mar. 18, 1890
SPRAGUE, FRANK J.	(*Vice-President.*) Electrical Engineer and Inventor, 182 West End Ave., Firm of Sprague, Duncan & Hutchinson, 15 Wall St., New York City.	May 24, 1887
SPRUSON, WILFRED J.	Member of the firm of Hepburn & Spruson, Consulting Engineers and Electricians, 169 King St., Sydney, Australia.	Dec. 16. 1890
SQUIER, GEORGE O.	Lieut. U. S. A., Student of Physics, Box 243. Johns Hopkins University, Baltimore, Md.,	May 19, 1891
STADELMAN, WM. A.	Vice-Prest. and Chief Engineer, Equitable Engineering and Construction Co., Drexel Building, Philadelphia, Pa.	Feb. 7, 1890
STANLEY, WILLIAM, JR.	Electrician, Pittsfield, Mass.	Dec. 6, 1887
STEARNS, CHARLES K.	Sup't of Construction, N. W. Thomson-Houston Electric Co., 403 Sibley St., St. Paul, Minn.	Aug. 6, 1889
STOCKBRIDGE, GEO. H.	Patent Attorney, and Editor *Electric Power* 136 Liberty St., New York.	May 24, 1887
STOCKLY, GEO. W.	President, Brush Electric Co., Cleveland, O. Lakewood, N. J	April 15, 1884
STONE, CHARLES A.	Manager with E. S. Webster, Mass. Electrical Engineering Co., 4 P. O. Square Boston. Mass.	May 19, 1891
STRONG, FREDERICK G.	Electrician, Midland Electric Co., 1616 Seventeenth St., Denver Colo.	Oct. 27, 1891
STUART, OTIS K.	Agent, Germania Electric Co., Room 21, City Trust Building, Philadelphia, Pa.	June 4, 1889
STUMP, CLARENCE E.	Vice-President and Business Manager, Street Railway Publishing Co., World Building, New York.	May 17, 1887
SULLIVAN, M. C	Electrical Engineer, care Electric Club, 17 E. 22d St., New York City.	Dec. 16 1890
SWEET, HENRY N.	Chief of Patent Bureau, Thomson Electric Welding Co., 89 State St., Boston, Mass	May 20 1890
TABER, ROBERT B.	Gas Engineer, Special Agent Thomson-Houston Electric Co., 620 Atlantic Ave., Boston, Mass.	Sept. 16, 1890
TAYLOR, CHARLES	Metallurgist, U. S. Assay Office, 30 Wall St., New York.	Nov 1, 1887
TEMPLE, WILLIAM CHASE	Mechanical and Electrical Engineer, Lewis Block, P. O. Box 800 Pittsburgh, Pa.	May 3, 1887
TESLA, NIKOLA	Electrical Engineer and Inventor, The Gerlach, 55 West 27th St., New York.	June 5, 1888

Name.	Address.	Date of Election.
THOMPSON, ALEXANDER	Supt. Electrical Dept., and Chief Engineer, Superior Water, Light and Power Co., West Superior, Wis.	April 21, 1891
THOMPSON, WILLIAM GEO. MACNEILL,	Resident Engineer, Sault Ste Marie Canal, Sault Ste. Marie, Ontario.	July 12, 1887
TOBEY, WILLIAM BOARDMAN	Pittsfield Electric Light & Power Co., Pittsfield, Mass.	Sept. 16, 1890
TOTTINGHAM, F. M.	Supt. 2nd. District, Edison Electric Ill. Co., 49 W. 26th St., New York City.	April 21, 1891
TOWNSEND, HENRY C.	(*Manager*). Attorney and Expert in Electrical Cases, 5 Beekman St., New York.	July 10, 1888
TREGONING, JOHN	32 Belmont Ave , Providence, R. I.	April 2, 1889
TROTT, A. H. HARDY	Electrical Expert, with Thomson-Houston Electric Co , Lynn. Mass.	Jan. 20, 1891
TUTTLE, GEORGE W.	Storekeeper, Sawyer-Man Electric Co., 510 West 23d St., New York City.	Mar. 17, 1891
UEBELACKER, CHAS. F.	Brush Electric Co., and 1299 Euclid Ave., Cleveland Ohio.	Feb. 7, 1890
UEHLING, T. A.	Ass't. Supt. and Chemist, U. S. Aluminium Metal Co., Boonton, N. J.	May 19, 1891
UHLENHAUT. FRITZ, JR.	Field Engineering Co., 143 Liberty St., New York	May 7, 1889
UPTON, FRANCIS R.	(*Manager*). Treasurer and General Manager, Edison Lamp Co., Harrison, N. J. Residence, 107 Day St., Orange, N. J.	May 17, 1887
VAIL, THEO. N.	18 Cortlandt St , New York.	April 15, 1884
VAN BRUNT, WALTER	Manager, Duluth Telephone Co., Duluth, Minn.	Sept. 6, 1887
VANCE, A. ST. CLAIR	General Superintendent, The H. Ward Leonard & Co., 136 Liberty Street, New York City.	April 2, 1889
VANDERGRIFT, JAMES A	Assistant Supt. and Electrician, Sawyer-Man Electric Co., 534 W. 23d St., New York City.	Nov. 24, 1891
VAN TRUMP, C. REGINALD	Wilmington City Electric Co , Wilmington, Del.	Feb. 5, 1889
VAN VALKENBURGH, F. S.	Central and South American Tel. Co , Tehuantepec, Mexico.	June 5, 1886
VAN VLECK, FRANK	Executive Engineer, Pacific Railway Co , Los Angeles, Cal.	Nov. 16, 1886
VAN WYCK, PHILIP V. R., JR.	36 W. 12th St., New York City.	April 21, 1891
VARLEY, RICHARD, JR.	Electrician, Okonite Insulated Wire Co., Passaic, N. J.	Mar. 18, 1890
VIDUARRE, JOSE	Dealer in Electrical Supplies, Ponce, Porto Rico.	Sept. 6, 1887
WACKER, GEORGE G.	Electric Organs, 3644 Third Ave., New York.	Sept. 6, 1887
WALLACE, WILLIAM	Wire Manufacturer, Ansonia, Conn.	April 15, 1884
WALTER, HENRY E.	3 Princes Mansions, Victoria St., London, Eng.	April 2, 1889

Name	Address	Date of Election.
WARDELL, GEORGE P.	19 Cedar St., Newark, N. J.	Nov. 12, 1889
WARING, RICHARD S.	205 Penn Building, Pittsburgh, Pa.	April 15. 1884
WARING, JOHN	Consulting Electrician, The Perkins Electric Lamp Co., Manchester, Conn.	Dec. 16, 1890
WASON, CHAS. W.	Electrical Engineer, East Cleveland R. R. Co., 1762 Euclid Ave., Cleveland, O.	May 19, 1891
WATERHOUSE, FRANK G.	Room 6, No. 302 Asylum St., Hartford, Conn.	Sept. 6 1887
WATERS, EDWARD G.	Electrical Engineer. Thomson-Houston Electric Light Co , German National Bank, Pittsburg, Pa.	Mar. 18, 1890
WATSON, ROBERT	The Elektron M'f'g. Co., 89 Liberty St., New York City.	Oct. 21, 1890
WATTS, H. FRANKLIN.	Electrical Engineer, 350 Edison Ave., Schenectady, N.Y.	May 20, 1890
WEBSTER, EDWIN S.	Mass. Electric Engineering Co., 4 P. O. Square Boston, Mass.	April 21, 1891
WELLES, FRANCIS R,	Manufacturer, Bell Telephone Manufacturing Co., Antwerp, Belgium	Sept. 6, 1887
WHITE, GEO. MONTAGU	Agent for West Indies, Thomson-Houston International Electric Co., Kingston, Jamaica, W. I.	Sept. 22, 1891
WHITE, H. C.	Manager, Phœnix Iron Works Co., 15 Cortlandt St., New York.	April 15, 1884
WHITE, J. G.	J. G. White & Co., Electrical Engineers and Contractors, 29 Broadway New York City.	April 2, 1889
WHITE, WILL F.	Electrical Engineer, Sec'y and Treas. Western Engineering Co., 415 So. 15th St., Omaha, Neb.	Feb. 7, 1890
WHITMORE, W. G.	Electrical Engineer, Edison General Electric Co., Edison Building, Box 3067, New York City.	Mar. 18, 1890
WHITNEY, HENRY M.	President, West End Street Railway Co., 81 Milk St., Boston, Mass.	July 12, 1887
WIGHTMAN, MERLE J.	Vice-President and Electrician, The Wightman Elec. Mfg. Co., 4 Commonwealth Bldg., Scranton, Pa.	Mar. 5, 1889
WILEY, WM. H.	Scientific Expert, 53 East 10th St., New York.	Feb. 7, 1888
WILLIAMS, ARTHUR S.	Electrician, American Telephone and Telegraph Co., 18 Cortlandt St., New York City.	Nov. 24, 1891
WILLIAMS, CHARLES, JR.	Electrician, 100 Sudbury St., Boston, Mass.	April 15, 1884
WILLIAMS, JAMES B *M. D.*	44 Broadway, Room 60, New York.	Sept. 7, 1888
WILSON, CHARLES H.	General Superintendent, Chicago Telephone Co., 203 Washington Street, Chicago, Ill.	Nov. 24, 1891
WILLYOUNG, ELMER G	Electrician, James W. Queen & Co , 924 Chestnut Street, Philadelphia, Pa.	Nov. 24, 1891
WINKLER, CHARLES F.	Electrician, Troy Electric Dynamo Co., 4 Park Ave., Troy, N. Y.	Sept. 3, 1889
WINSLOW, I. E.	458 Produce Exchange, New York.	Nov 12, 1889

Name.	Address.	Date of Election.
WINTRINGHAM, J. P.	Theorist, 36 Pine St., New York.	May 7, 1889
WIRT, CHARLES.	Electrical Engineer of the Electrical Supply Co., 102-104 Michigan Ave., Chicago, Ill.	Sept. 8, 1888
WIRT, HERBERT C.	Electrician, Marine Dept. Thomson-Houston Electric Co., 12 Millmont St., Roxbury, Mass.	June 26, 1891
WOLVERTON, B C.	Electrician, N. Y. & Pa. Telephone & Telegraph Co., Elmira, N. Y.	Mar. 18, 1890
WOOD, E. J.	Consulting Engineer and Contractor, 243 Broadway, New York.	July 12, 1887
WOODRUFF, H. O.	Des Moines, Iowa.	Oct. 2, 1888
WOODWARD, FRANCKE L.	Student in Electricity, 49 Grand St., Albany, N. Y.	June 26, 1891
WOOLF, ALBERT E.	Electrician and Inventor, Woolf Electrical Co., 20 Nassau St. Room 50 and 864 Lexington Ave., New York City.	Sept 16, 1890
WORTHINGTON, GEORGE	Buffalo, New York	April 15, 1884
WRIGHT, JOHN D.	Electrician's Assistant, United Electric Traction Co., 17 East Park St., Newark, N. J.	Oct. 21, 1890
YOUNG, ALDEN M.	Contractor for Organization and Complete Equipment of Electric Lighting Plants, 101 Bank St., Waterbury, Conn.	Sept. 6, 1887
ZALINSKI, EDMUND L.	Captain of Artillery, U. S. A., Care of War Dept., Washington, D. C.	May 17, 1887

Associate Members, - - - 408.

OFFICIAL STENOGRAPHER.

RYAN, RICHARD W., 300 Mulberry St., New York City.

SUMMARY.

Honorary Members, - - - - - - - - -	3
Members, - - - - - - - - - - - -	172
Associate Members. - - - - - - - -	408
Total - - - -	583

NON-CIRCULATING

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